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THE CONSTANT ERROR OF TOUCH LOCALIZATION

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In the consideration of the ability of localization on the skin of points stimulated by light touches, by pressures, by pain stimuli, and by temperature stimuli, the conclusions of Ponzo[1] have been widely quoted and accepted.[2] The conclusions set forth by Ponzo in his various articles are, as I have shown in a previous publication,[3] open to question in certain particulars, and it is with regard to his conclusions respecting the constant error that the present paper is presented. It is convenient to deal with these matters under the two headings of method and facts.

Method.—The usual method of determining the constant error in experiments of this character is familiar to all who have investigated these matters. It does not need to detain us long at this time. Briefly the constant error is found in the following manner: The tendency to the localization of points in a special direction is determined by referring the localizations to predetermined axes of ordinates which meet at the point of reference or origin of coördinates, which in touch localization tests is the point stimulated. The points localized to the right and those to the left of the vertical axis are considered algebraic opposites, and similarly those

[1] M. Ponzo, 'Recherches sur la localisation des sensations tactiles et des sensations dolorifiques,' *Arch. ital. de biol.*, 1911, 55, 1–14.

[2] See, for example, the reviews of H. D. Cook in *Psychol. Bull.*, 1913, 10, 258–261; 1914, 11, 238–241.

[3] S. I. Franz, 'The Accuracy of Localization of Touch Stimuli on Different Bodily Segments,' *Psychol. Rev.*, 1913, 20, 107–128.

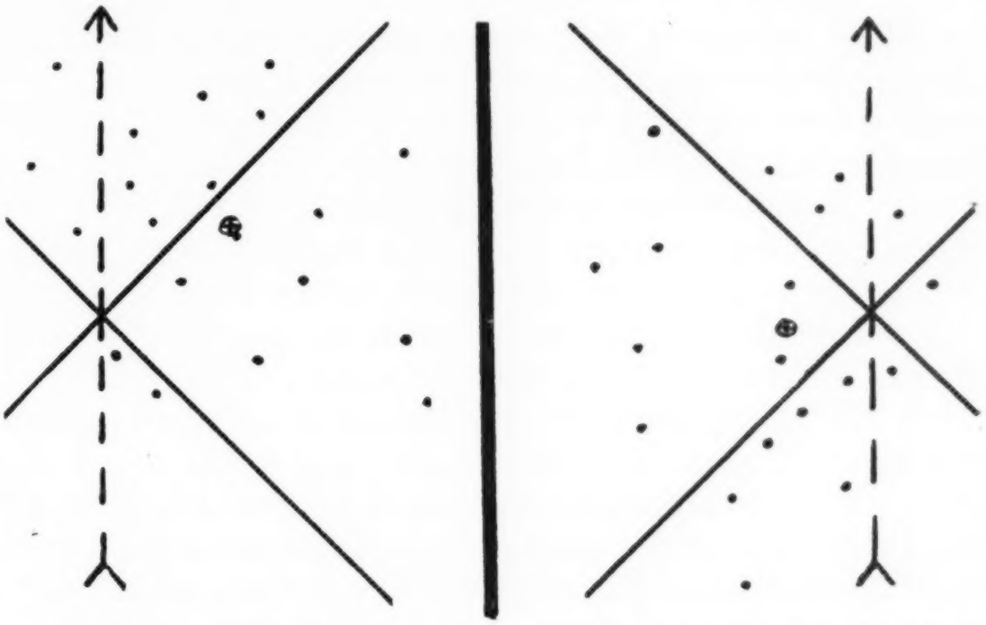
above and those below the horizontal axis. The averages of the algebraic sums will give the location of the average tendency, or the average constant error.[1]

Ponzo's method of determining the constant error is, superficially, one of great simplicity, and of apparent value. Briefly in relation to the localization of the skin sensations it is as follows: Either before or after the performance of a series of tests a primary axis is drawn through the point of stimulation to represent the supposed direction of the nerve or other anatomical segmental axis; through the point two perpendicular lines are drawn in such a fashion that each cuts the primary axis at an angle of 45 degrees; the right angles which include the primary axis are then considered by Ponzo to be boundaries to those points which are in the direction of the primary axis and, consequently, to be in the direction of the anatomical part. All localizations falling within these boundaries are considered to be "in the direction of the primary axis."

A brief consideration will show that by the use of Ponzo's method conclusions may be reached which are neither accurate nor representative of the actual conditions. The extreme case of the points of localization falling close to one of the boundaries or axes of reference might show the constant error falling within the direction of the primary axis, and with a slight shifting of the points the constant error may be found to be in a direction at right angles to the primary axis. Ponzo's method breaks down in these extreme cases, and in cases which are not extremes. If the latter can be shown, we should conclude that the method is not sufficiently exact, and that conclusions based upon the method should not be accepted until they have been confirmed by facts obtained by further and more exact methods. Two illustrations will suffice to indicate the discrepancies in the conclusions to be drawn from certain results when the two methods of determining the constant error are used. The results which give these illustrations are shown in Figs. 1 and 2.

[1] These matters are so elementary that at first sight it seems superfluous to mention them, but the acceptance of Ponzo's conclusions, without due criticism, would indicate the necessity for repetition.

They show the localizations of a subject when two points on the mid-body lines were stimulated.



FIGS. 1 AND 2. Results of experiments on the localization of tactile stimuli. The points stimulated are at the intersections of the lines. The dotted lines show the principal axes, full lines, the general directions according to Ponzo. Each localization is shown as a point, the location of the average constant error as an encircled cross. Twenty tests in each experiment. For further details see texts.

In these diagrams the primary axis is shown by a broken line (with arrow markings), and Ponzo's constant error indicators are shown as unbroken lines. Each point of localization is shown as a dot, there being twenty in each illustration. Both experiments show according to Ponzo's method a superiority of localization in the direction of the primary axis. In Fig. 1 the number of points in the space 315° to 45° is 10, and in the corresponding space, 135° to 225°, 2; the numbers in the perpendicular spaces, 45° to 135° and 225° to 315° being 8 and 0 respectively. In Fig. 2 the points in the principal directions, 315° to 45° and 135° to 225°, are 5 and 7 respectively, while in the perpendicular spaces, 45° to 135° and 225° to 315°, there are 1 and 7 respectively. In each of these experiments, therefore, there are 12 points in the direction of the principal axis and 8 points not in that direction. It will thus be seen that there is an apparent

constant error (by Ponzo's method) in the direction of the principal axis, the number of such localizations being fifty per cent. more than in the other direction.

When the results in these two experiments are calculated by the usual method of determining the constant error the supposed tendency to locate along the principal axis is not found to be present. Thus, the results in Fig. 1 show an average constant error toward the right of 15.2 mm. and an upward tendency of 10.6 mm. The results in Fig. 2 show an average constant error of 11.2 mm. to the left and 1.9 mm. downward. The localizations of these average constant errors are shown by small encircled crosses.

It will be seen, therefore, that although by Ponzo's method the constant error in these two examples is the same, this error when determined by the usual method differs in the two cases. In the first example it is toward the right, in the second it is toward the left. In the first example the constant error is at an angle of 55.5 degrees from the upper part of the principal line, in the second it is at an angle of 260 degrees.[1] In each case the "angular" error does not lie within the angular limits which Ponzo sets as the direction or axis of the anatomical part.

An objection may be raised that the illustrated examples are isolated and extremes, and that they do not fairly represent the conditions with which Ponzo, in his work, and I, in my former paper, dealt. In a brief space it is not possible to discuss in detail the results which Ponzo has reported and illustrated, but examples in which similar, but perhaps not as extensive, differences are to be found are given in Ponzo's publications. At the same time it may be stated that the results on the individual points which were grouped in my previous article show at times as great deviations as in the two examples given here. Because of these variations in a number of tests on different days the average constant error for one point or for an anatomical segment is not large. Furthermore it should be stated that if the method of Ponzo

[1] Both roughly measured by a protractor showing angular degrees, and using the vertical axis above the stimulated point as zero degrees.

breaks down in cases of this character, which I cannot consider to be extremes, it is a method upon which little or no reliance can be placed, especially when broad generalizations are to follow from the account of the results.

Facts.—A question immediately arises regarding the accuracy of Ponzo's statement of the conditions encountered in localization tests when the adequacy of his method of dealing with the observations is disputed. Are suppositious general tendencies toward localizations along certain bodily axes, or along the courses of nerves, dependent upon his erroneous method of calculation of the constant error, or are they found to exist when the usual and more accurate constant error method is employed? An answer to this question is given in part in my previous paper, although in that article the results of the constant errors of localizations of touch stimuli for individual points were not given in full. The fact that my results on the individual points were not reported has led some to believe that my groupings into larger anatomical segments have concealed or balanced the constant errors, and that the apparent discrepancies between Ponzo's and my conclusions would disappear if I had dealt with the individual points. A further consideration of the groupings and the results from them will, however, serve to show that such is not entirely, if at all, the case. If similar groupings are made for all subjects the results should be directly comparable, regardless of the kinds of groupings, and it would make no difference how large or how small a grouping is made. That the constant errors are not the same in the two subjects, *A* and *C*, whose results were published, is evident from inspection.[1] At the same time it must be observed that with the groupings in the two series of the same subject, *A*, the constant errors were not always the same. Other results to which I may refer are those for the soles of the feet of these two subjects, on each of which anatomical part only one point of stimulation was used. The results of the two series of tests on subject *A* do not correspond, although the results with subject *C* have a fairly close correspondence with

[1] See Table V., page 120 of my previous article, *op. cit.*

the results of the first series on subject *A*.[1] In this connection attention may be called to the results obtained by another method of constant error estimation which I employed and which gave inconstant results. When the subject localized the first stimulation this localization was taken as the second point of stimulation, the second localization was taken as the third point of stimulation, etc. Subject *A* by this method, starting from a point on the mid-line of the body and on a level with the nipples, reached in 10 tests on one day "a point 8.5 cm. from the mid-line and 17 cm. below the first stimulation. . . . In a second experiment on a subsequent day in 10 tests the point was reached on the left 10 cm. from the mid-line and 16.5 cm. below the first stimulation. In a third series, a point was reached after 10 tests, 11 cm. from the mid-line, and 9 cm. below the first stimulus."[2] In relation to the primary points the general tendencies in these three tests are indicated by the angular relations of the average constant errors to the point on the mid-line. These are respectively 153, 148, and 129 degrees.[3] The approximate amount of the angle of the ribs with the sternum at the original point of stimulation is approximately 85 degrees. These results tend to confirm what I shall now endeavor to show more accurately, namely, that the errors of localization of stimuli to definite points in certain anatomical segments

[1] It would not be justifiable to make further comparisons of supposedly similar points on different subjects at this time, for it is not possible to determine that the points were in all cases anatomically identically located. The subjects are no longer available (artists' models make up part of our city-to-city floating population) and anatomical comparisons cannot be made. The reason why this comparison cannot be made from my available records is because I selected for my former work points which were at definite distances from certain anatomical landmarks, such as the axilla, the umbilicus, the nipples, etc., but with the differences in lengths and girths of the bodies the points did not always correspond with respect to other bony landmarks and with the supposed courses of nerves. Especially in relation to the stimulated points on the thorax are these differences of importance, for a variation of one or two centimeters in the length of the chest would shift the location of a point from an intercostal space to a supracostal area, or vice versa. The recalculation of the individual points would not, therefore, be of any advantage towards the solution of the problem.

[2] *Op. cit.*, p. 121.

[3] Protractor measurements.

do not correspond with the direction of the nerves, as Ponzo has contended.

Ponzo's[1] conclusions on this point refer especially to the localization of pressure and pain stimuli in the intercostal spaces. He has reported for three subjects results of localization of stimuli in the fifth, sixth, seventh, and eighth intercostal spaces, which are interpreted to indicate a localization in the direction of the intercostal spaces, with the further tendency to locate toward the end branches of the nerve which innervates the skin covering such a space. He subsequently added the conclusion that the errors of localization of sensations of warmth and of cold on other bodily parts correspond with the general direction of the superficial nerve trunks.[2] The value of these conclusions, if they should be found to be correct, is very great for anatomical workers, but greater for clinicians who, by the application of a few tests to show the constant error in the localization of pressures and temperature stimuli, would be enabled to discover in the living subject the course of the cutaneous nerves in different portions of the body.

The present series of tests were made upon an artists' model, Miss W., who is about 20 years of age.[3] Special attention was paid to certain areas of the body in which the directions of the nerves are very constant, in order that the constant error tendencies might be considered in relation to the courses of these nerves. The areas which were investigated are as follows: (*A*) A point in the axillary line overlying the seventh intercostal space when the arm is lying fairly close to the body; (*B*) a point near the mid-line of the chest at the junction of the fourth intercostal space with the sternum; (*C*) a point on the mid-body line, at the level of the nipples; (*D*) a point on the anterior part of the thigh,

[1] Ponzo, M., 'Observations sur la direction des erreurs de localization dans les espaces intercostaux,' *Arch. ital. de biol.*, 1911, 56, 193–201.

[2] Ponzo, M., 'Studio della localizzione delle sensationi termiche caldo e freddo,' *Riv. di psicol.*, 1913, 9, 393–415.

[3] In most of the tests I have had the assistance of Dr. Mildred E. Scheetz, who also in my absence carried out some of the tests which are here reported. The results obtained by her did not differ from those carried out by me more than those obtained by me on one day differed from those obtained on a subsequent day.

about half way from the knee and about midway between the middle and internal branches of the anterior cutaneous nerve of the thigh (branches of the femoral or anterior crural); and (*E*) a corresponding point on the lateral portion of the thigh, which is supposedly supplied by the lateral femoral cutaneous nerve. All of the nerves supplying these parts are relatively constant in direction, although in the living subject it is not possible to determine their exact courses.[1] Dissection shows them to be about as constant in location as any other nerves of the body, and they were selected instead of arm nerves because of the presumably lesser experience the subject would have in locating stimuli along their courses. It should not be supposed, however, that the location of skin areas in relation to the underlying nerves can be determined exactly, and that the selected points are definitely related to the supposed underlying nerve trunks. With respect to the points at the intercostal spaces, and *pari passu* the remark applies to the other locations, a slight amount of movement of the arm or a slight twist of the thorax will throw the superficial point out of its definite relation to the intercostal space, bringing it above a rib.[2]

The stimuli were given by means of a short brush, stiffer than one which I have previously used for light touch tests, and it is certain that not only was the superficial skin stimulated but also the underlying tissues. This would give results relating to the compound of touch and pressure stimuli, which corresponds well with the conditions in the work of Ponzo. Twenty tests were made at each point on one day, and the averages of the tests are considered here. At the same time it is possible to combine the tests on different

[1] The courses of these nerves may be learned from almost any good text-book of anatomy.

[2] This is a point which must be considered in dealing with the relations of localizations to underlying nerves. What for example shall we consider the nerve supply to the skin overlying an intercostal space to be? Is it from the nerve emerging in the intercostal space which the skin covers when the arm is held close to the side of the body? Or, is it the nerve of the space above, which the skin covers when the arm is raised above the head or in some cases when the arm is at a right angle to the axis of the trunk? In the present work the superficial assumptions of Ponzo have been followed, but not necessarily accepted.

days into suitable groups referring to the same point, and to make comparisons of the results.

Seventh intercostal space.—Two series of experiments were made on the left side, and one on the right, in the seventh intercostal space where the space meets the mid-axillary line. Twenty tests were made in each experiment. The results of the calculation of the constant errors in these experiments are as follows: On the left, 25.3 mm. upwards, 3.1 mm. toward the back of the body; and 4.0 mm. downwards, and 1.9 mm. toward the back of the body; on the right, 7.5 mm. upwards, and 14.1 mm. toward the front of the body. It will be noted that the error away from the axillary line, toward the front or back, differs in the experiments on the right and left sides, and it will also be noted that the errors in the two series on the left side differ with respect to the location of the stimulated point in relation to the upper and lower (cephalad and caudad) portions of the body. It is apparent that by the usual methods of determining the constant tendency there is not much agreement among the three experiments. With reference to the angles of the average constant error localizations in relation to the mid-axillary line it may also be said that in the three experiments the localizations are different. They are respectively 7°, 155°, and 61.5°.

At the times the tests were made the directions of the intercostal spaces in relation to the mid-axillary line were determined by laying a finger upon the skin overlying the space and after having pressed the finger into the space as closely as it would fit the outlines were marked upon the skin with a grease pencil. The center line between the two lines which were marked was taken to represent the direction of the space, and its angular direction in relation to the mid-axillary line was determined, using the portion of the latter above the point as zero degrees. In the three experiments which have been described the directions which were determined were greatly different, for they were 120°, 137°, and 122° respectively. At first glance there appears the probability of great error in the making of these measurements,

for it seems improbable that on the same subject the angle of the ribs should differ by as much as these measurements differ. To test the matter the subject was at a subsequent session measured a number of times, both by me and by an assistant. The grease pencil marks were obliterated after each record was taken and new marks were made for each determination. The results of the measurements are as follows: Right side, with the arm close to the body, 119° and 118°, with the arm abducted and held above the head, 114° and 131°, left side, with the arm at the side of the body, 130° and 120°, with the arm raised above the head, 128° and 125°. These measurements which were taken with extra care do not differ from those taken three months previously more than do the earlier corresponding measurements from one another. In other words it appears that the angular relation of the seventh intercostal space to the mid-axillary line is not constant, or that the errors of measurement are large. The former is what might be expected from what we know of the rib movements in connection with the acts of respiration. At the same time it should also be noted that the angular directions of the intercostal spaces vary according to the position of the arms. This is shown by the series of figures just given and is further demonstrated by the results of a series of measurements on other subjects on whom the angular measurements were taken by Dr. Scheetz. These measures are given in Table I.

This table shows clearly the variations in accordance with the positions of the arms, the averages for all completed series (two each on *A* and *B*, and one each on *C* and *D*) being 95° when the arm is held close to the body, 111° when the hand is placed upon the opposite shoulder, and 118° when the arm is raised above the head. These variations, it will be noted, are greater than those found with the subject on whom the present tests were made, and may partly be accounted for by differences in the shapes of thorax, and in the amounts of subcutaneous fat in the four subjects.

Assuming, however, that the angular measures first taken on our present subject (120°, 133°, and 122°) are sufficiently

exact, we may examine the results of the localizations by the method of Ponzo. When we draw the bounding lines to indicate the directions we find for the three series the following: I., In the direction of the space, 5 localizations, away from the direction of the space, 15 localizations; II., in the direction of the space, 8, away from the direction, 12; III., in the direction, 8, away from the direction, 12. In no case, therefore, did the subject show a constant tendency to localization in the direction of the space, as has been contended.

TABLE I

ANGULAR DIRECTIONS OF THE SEVENTH INTERCOSTAL SPACES IN RELATION TO THE MID-AXILLARY LINES

Asterisks denote that measurements were not made in these cases.

Subjects	Sides of Body	Arm Near Body	Hand on Opposite Shoulder	Arm above Head
A.....	R.	91	112	112
	L.	108	122	135
B.....	R.	109	120	121
	L.	78	117	106
C.....	R.	89	114	127
	L.	*	103	104
D.....	L.	95	83	107
E.....	R.	126	*	*
	L.	118	*	*

Fourth Intercostal Space.—One series of twenty experiments on a point in this space, on the right side four centimeters from the mid-line, gave an average constant error 11.8 mm. downwards, and 14.1 mm. toward the mid-line of the body. The angular relation of the space to the mid-body line was found to be approximately 90°. The angular location of the constant error is, therefore, within the boundaries set by Ponzo (45° to 135°). At a subsequent session measurements of the angular relation of the space to the mid-body line gave 78° on the right side, and 87° on the left side. In another subject the angular relation was 86°. It will be seen, therefore, that the error is close to the lower boundary of the angular limits, although within them.

When measured in relation to the boundary lines it was found that 14 of the localizations were within the boundaries

and 6 outside, although it should also be mentioned that only two of the localizations were between 0° and 90°, and the remaining 18 were in the space bounded by the 90° and 180° lines.

Mid-body Line at the Level of the Nipples.—Two tests of twenty experiments each were made with the right hand for localization and one similar test with the left hand for localization. These three tests show respectively the following constant errors: I., 7.8 mm. downwards, 15.2 mm. toward the left side; II., 0.9 mm. toward the head, 6.1 mm. toward the left; III., 6 mm. toward the head, 3.7 mm. toward the left. The results in the three series correspond as far as left-sided localization is concerned, but do not correspond with respect to cephalad or caudad localizations. Since we have no definite knowledge regarding the course of the nerves at this point beyond the general belief that the skin is probably innervated almost equally from both sides of the spinal cord, the results are negative. It should be said, however, that if we believe in an equilateral innervation there is no apparent reason for the left-sided localization constant error.

Anterior Thigh.—The location of the stimulated point has already been mentioned. The results of 200 experiments, 20 on each leg on each of five days, are given in the accompanying table (Table II.) which shows the average errors of localization, the constant errors centralwards or distalwards, and to the right or to the left. The results of the calculation of the same results by the rougher method of angular limits are also shown in Table III. It will be seen that the constant errors in the five series do not correspond. In one test the average constant error lies to the left, in another test it lies to the right; in one it is toward the abdomen, in another it is toward the foot. At the same time it will be observed that there are great variations in the localizations in relation to the angular measurements, and that, on the assumption of a direction of nerve from 355° to 175°, the constant error is located within the angular limits in only seven of the ten experiments. The totals of the 100 experiments on the right and the 100 experiments on the left show that the

general direction of localization is in the nerve direction, if we can use the method of angular limits for the determination of the constant error. This tendency to location in the supposed direction of the nerve is, however, not very large, for there is only a superiority of 50 per cent. in the localizations in the direction as compared with those at right angle to the direction (viz., 120 : 80).

TABLE II

AVERAGE AND CONSTANT ERRORS ON THE RIGHT ANTERIOR THIGH. 20 EXPERIMENTS ON EACH DAY

Constant errors calculated by the usual method.

	Serial Days					Averages
	1	2	3	4	5	
Av. error	24.3	17.5	12.0	20.0	17.0	18.2
C.E. ⇅	↑19.1	↑10.5	↓6.3	↓13.8	↓1.6	↑4.0
C.E. ⇄	0	0	←1.3	→5.3	→9.7	→2.8
C.E. degrees	0	0	168.5	159.0	99.0	35.0

AVERAGE AND CONSTANT ERRORS ON THE LEFT ANTERIOR THIGH. 20 EXPERIMENTS ON EACH DAY

Av. error	17.8	13.0	12.5	14.0	25.0	16.5
C.E. ⇅	↑3.3	↓2.8	↓4.8	↓2.7	↑4.0	↓1.5
C.E. ⇄	←2.8	→1.7	←2.1	←7.5	→0.2	←2.1
C.E. degrees	320.0	149.0	203.5	250.0	3.0	235.0

TABLE III

CONSTANT ERRORS DETERMINED BY METHOD OF ANGULAR LIMITS

Right Side

Serial Days	Direction of Nerve	Opposite Direction
1	13	7
2	11	9
3	14	6
4	10	10
5	15	5
Totals	63	37

Left Side

1	10	10
2	8	12
3	12	8
4	15	5
5	12	8
Totals	57	43

TABLE IV

AVERAGE AND CONSTANT ERRORS ON THE RIGHT LATERAL THIGH.
20 EXPERIMENTS ON EACH DAY

Constant errors calculated by the usual method.

	Serial Days					Averages
	1	2	3	4	5	
Av. error	17.0	13.3	14.0	33.8	54.5	26.5
C.E. ⇅	↥3.8	↓2.8	↑8.5	↓5.2	↑10.3	↑2.9
C.E. ⇄	→9.1	←3.5	→3.7	→34.1	→50.6	→18.8
C.E. degrees	67.0	231.5	24.0	98.5	78.5	82.0

AVERAGE AND CONSTANT ERRORS ON THE LEFT LATERAL THIGH.
20 EXPERIMENTS ON EACH DAY

Av. error	22.8	14.8	16.0	23.3	28.3	21.0
C.E. ⇅	↓15.3	↑2.8	↑2.8	↑5.9	↑4.2	↑0.1
C.E. ⇄	←12.3	←7.5	←5.7	←8.8	←22.8	←13.4
C.E. degrees	219.0	290.5	297.0	287.0	280.0	270.0

TABLE V

CONSTANT ERRORS DETERMINED BY METHOD OF ANGULAR LIMITS

When a point has been on a line it has been calculated as half each way.

Right Side

Serial Days	Direction of Nerve	Opposite Direction
1	12	8
2	9.5	10.5
3	10.5	9.5
4	0	20
5	0	20
Totals	32	68

Left Side

1	10.5	9.5
2	5.5	14.5
3	7	13
4	3.5	16.5
5	5	15
Totals	31.5	68.5

Right Lateral Thigh.—The nerve supplying this portion of the body is very constant in direction in cadavers, following an almost straight line from the crest of the ileum to the middle of the knee, in other words from 0° to 180°. The re-

sults of five tests on different days on the two legs on the selected points, 200 experiments in all, are given in Table IV., and the results of the calculation by the method of angular limits are given in Table V.

In these tables it will be observed that the constant errors in the individual tests differ greatly. In some there is an apparent tendency to locate toward the pelvis, in others toward the knee; in one there is a tendency to locate toward the dorsal surface and in the other nine toward the anterior surface of the leg. The angular relations of the constant error determinations to the supposed nerve course in these ten series greatly differ, ranging from 24° to 231° on the right leg, and from 219° to 297° on the left leg. Within the artificial angular limits there is only one constant error direction on the right and none on the left side, four on the right and five on the left being outside of the supposed angular directions.

In only three tests is there a predominance of localizations in the direction of the artificially set angular limits, and in the other seven tests there is a superiority of localization in the spaces at right angle to the artificially set directions. For the total of ten tests, therefore, there is a superiority in direction away from the space amounting to over 100 per cent.

Summary.—The method of calculation of constant error by angular limits is too inexact to warrant its use except as a possible qualitative method for preliminary work. The constant error tendency in the five locations selected for the present series of tests is in only one place in the supposed direction of the innervating nerve, whether this tendency be measured by the usual and more exact means or by the method of angular limits used by Ponzo. Conclusions regarding the constancy of direction of constant errors must be radically changed to conform to the facts.

One possible objection may be raised to the present series of results. This is that they have been obtained with only one subject. This objection loses its force in view of the widespread generalizations which have been made, for it

TABLE IV

AVERAGE AND CONSTANT ERRORS ON THE RIGHT LATERAL THIGH.
20 EXPERIMENTS ON EACH DAY

Constant errors calculated by the usual method.

	Serial Days					Averages
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Av. error	17.0	13.3	14.0	33.8	54.5	26.5
C.E. ⇅	↥3.8	↓2.8	↑8.5	↓5.2	↑10.3	↑2.9
C.E. ⇄	→9.1	←3.5	→3.7	→34.1	→50.6	→18.8
C.E. degrees	67.0	231.5	24.0	98.5	78.5	82.0

AVERAGE AND CONSTANT ERRORS ON THE LEFT LATERAL THIGH.
20 EXPERIMENTS ON EACH DAY

Av. error	22.8	14.8	16.0	23.3	28.3	21.0
C.E. ⇅	↓15.3	↑2.8	↑2.8	↑5.9	↑4.2	↑0.1
C.E. ⇄	←12.3	←7.5	←5.7	←8.8	←22.8	←13.4
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TABLE V

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When a point has been on a line it has been calculated as half each way.

Right Side

Serial Days	Direction of Nerve	Opposite Direction
1	12	8
2	9.5	10.5
3	10.5	9.5
4	0	20
5	0	20
Totals	32	68

Left Side

1	10.5	9.5
2	5.5	14.5
3	7	13
4	3.5	16.5
5	5	15
Totals	31.5	68.5

Right Lateral Thigh.—The nerve supplying this portion of the body is very constant in direction in cadavers, following an almost straight line from the crest of the ileum to the middle of the knee, in other words from 0° to 180°. The re-

sults of five tests on different days on the two legs on the selected points, 200 experiments in all, are given in Table IV., and the results of the calculation by the method of angular limits are given in Table V.

In these tables it will be observed that the constant errors in the individual tests differ greatly. In some there is an apparent tendency to locate toward the pelvis, in others toward the knee; in one there is a tendency to locate toward the dorsal surface and in the other nine toward the anterior surface of the leg. The angular relations of the constant error determinations to the supposed nerve course in these ten series greatly differ, ranging from 24° to 231° on the right leg, and from 219° to 297° on the left leg. Within the artificial angular limits there is only one constant error direction on the right and none on the left side, four on the right and five on the left being outside of the supposed angular directions.

In only three tests is there a predominance of localizations in the direction of the artificially set angular limits, and in the other seven tests there is a superiority of localization in the spaces at right angle to the artificially set directions. For the total of ten tests, therefore, there is a superiority in direction away from the space amounting to over 100 per cent.

Summary.—The method of calculation of constant error by angular limits is too inexact to warrant its use except as a possible qualitative method for preliminary work. The constant error tendency in the five locations selected for the present series of tests is in only one place in the supposed direction of the innervating nerve, whether this tendency be measured by the usual and more exact means or by the method of angular limits used by Ponzo. Conclusions regarding the constancy of direction of constant errors must be radically changed to conform to the facts.

One possible objection may be raised to the present series of results. This is that they have been obtained with only one subject. This objection loses its force in view of the widespread generalizations which have been made, for it

becomes apparent that if the conclusions set forth by the Italian investigator are not applicable to other than his own subjects they cannot be accepted as general. I am not willing, however, to admit that Ponzo's conclusions have been founded upon the utilization of the best methods, and in fact it is amply demonstrated that the method employed by him is so faulty that its use is unjustified.

THE COMPLICATION EXPERIMENT AND THE AFTER IMAGE

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I

The career of the 'Complication Experiment' has been so long and varied that the new investigator needs to spend a large portion of his time in repeating and confirming the results of earlier observers. To do this in the present instance with anything like adequacy, we have, for example, made over twenty-three hundred separate determinations. None of these was used in the material presented. In this case, the repetition of earlier investigations has been particularly interesting. In the development of the problem, one point after another has been discovered, yet new variations and new factors continue to appear. The following paper does not attempt an historical survey of the field. Such work has been performed with relative completeness by Sanford,[1] and more recently by Burrow.[2] Moreover, notwithstanding Dunlap's remark concerning the 'remnants of the complication-problem,' we have not convinced ourselves that sufficient work has yet been done to write its final history.

Disregarding the chronological order, the systematic history of the problem falls roughly into three divisions, the period of discovery and collection of data, the period of experimental modification of objective conditions, and finally that of specific directions to the observer. The first period begins with the discovery by Bessel, of the note in the Greenwich Observatory records, and again with Wundt's introduction of the problem into the psychological laboratory.

[1] Sanford, E. C., 'The Personal Equation,' *Amer. Jour. of Psychol.*, Vol. 2, pp. 3, 271, 403.

[2] Burrow, N. T., 'The Determination of the Position of a Momentary Impression, etc.,' *Psychol. Rev. Mon. Suppl.*, Vol. 11, No. 47.

The second is again begun by both Bessel and Wundt when the first mentioned changed the rate of the clock to test the effects on the observations, and the second, complicated the experiment by using the pendulum in place of the uniform movement of the clock hand. In the psychological study of this problem, as against its astronomical study, it is probable that Angell and Pierce[1] are the first investigators to give specific directions to their observers. Typical of the further development of this change in the method of experimentation is the 'inversion of the complication experiment' introduced by Stevens,[2] the rigid set of directions given his subjects by Dunlap,[3] to control eye movements, and the analysis of the methods of observation by Guenther,[4] and Dunlap.[5]

In the following pages, the results of some experiments with Dunlap's 'exact fixation' method, using practically an exact duplicate of Geiger's apparatus,[6] are presented. After practice with this method, our observers settle down to one or the other form of error with surprising steadiness. Our introspections show that the after image is a recognizable influence in making the determinations. By a slight modification in the apparatus which shortens the after flash of the pointer, the loss of this factor can be made to appear in the results. As Dunlap has shown the necessity of eliminating eye movements, we see here that a further condition for accurate results is some adequate visual cue to indicate the pointer position at the time of the click. The determinations that follow the selection of any single factor will be regular. Particularly the perception that follows the selection of the "pointer interruption" point on the scale is very accurate.

The most complete summary since Geiger, of the numerous

[1] Angell, J. R., and Pierce, A. H., 'Experimental Research on the Phenomena of Attention,' *Amer. Jour. of Psychol.*, Vol. 4, pp. 520–541.

[2] Stevens, H. C., 'A Simple Complication Pendulum for Qualitative Work,' *Amer. Jour. of Psychol.*, Vol. 15, p. 581.

[3] Dunlap, Knight, 'The Complication Experiment and Related Phenomena,' *Psychol. Rev.*, Vol. 17, pp. 157–191.

[4] Guenther, F., 'Reacktionsversuche bei Durchgangsbeobachtungen,' *Psychol. Stud.*, Bd. 7, S. 232 f.

[5] *Op. cit.*, p. 168 f.

[6] Geiger, M., 'Neue Complicationsversuche,' *Phil. Stud.*, Bd. 18, S. 347 ff.

factors involved in the judgment of coincidence, under its many and varied experimental conditions, has been given by Burrow. The point with which we are to deal first is treated by Dunlap.[1] He found that at the moment of the determination of coincidence the pointer did not blur, but remained frequently very distinct as if stationary. There could evidently be but one explanation for this,—the eye was making a short excursion with the pointer before settling down. The general observation of eye movement was noted by several other observers previous to this. Dunlap cast about for means to aid in maintaining this fixation point at the moment of coincidence. He finally used a screen of mosquito netting placed in front of the dial. By this means he was able to determine whether the eyes moved or not at the moment of alignment. If the threads of the net remained clear, it might be assumed that the fixation was adequate. His results indicate that inadequate fixation is perhaps the most potent factor in the production of errors. Other factors are still operative, but as he shows in a series of supplementary experiments, their effect is minimized when exact fixation is maintained. In taking records, he approached the assumed point of coincidence from both directions. This was done to equalize the influence of the two modes of approach, since Burrow had previously shown that they affect the errors differently.

II

After testing these results carefully, we decided to use one method of approach and to begin a practice series.[2] In this we hoped to discover more definitely the relation of the various factors involved. In place of the bell on the older apparatus, we used the click of a telephone receiver. This modification has been made in several recent experiments. Our scale, at first, was marked off with one-degree spaces; from series '11' on, a dial with scale marks two degrees apart and of equal length, was used. The five-degree divisions on

[1] *Op. cit.*, p. 164.

[2] Special mention is due Miss Mary Hill, since her continued assistance made possible the collection of a large portion of this data.

the first scale were longer than the degree marks, while the ten-degree divisions had marks approximately one centimeter in length and were numbered. The radius of the dial to the inner ends of the scale marks was 26 cm. The time of revolution was 1.2 sec. or $3\frac{1}{3}\,\sigma$ per degree. This rate was used throughout the following experiments.

The directions given all observers are as follows: "Fixate first the point at the top of the dial, marked 360." O. was warned to maintain exact fixation at all times and at the preliminary points to use only peripheral vision in determining tentative coincidences. "By successive fixations locate the point of coincidence." As far as possible, this was to be done by movement to the left only, that is, he was not to go 'negative' of the point of actual coincidence, though O. did not know, of course, whether he did this or not. By such successive approximations, he was expected to establish coincidence.[1]

In the first part of the experiment, we used only the left upper quadrant of the dial. Later we used the whole left half. All determinations were given in haphazard order. The points of actual coincidence were made out in advance and changed frequently so that O. would have no external cues to aid him. Fixation points are always to the right and above the sound in the upper quadrant. In the lower this is changed. Three different persons learned to manage the experiment, so that the position cue, or other cues, from E. might be made inoperative.

In the preliminary series mentioned above, we used over thirty different subjects. Only four subjects began the practice group arranged here; three others were added later. Subject I. knew the literature of the experiment thoroughly, while subject III. was unfamiliar with the nature of the experiments. Subjects II. and IV. were instructors in psychology. Subject III. did not take part in the preliminary experiments mentioned above.

The tables that follow are practically self-explanatory. The first column gives the number of the series; '1,' '2,' etc.,

[1] See Geiger's 'reflective type,' *op. cit.*, p. 378.

each constitutes a day's work for that subject. The columns headed 'Positive,' 'Negative,' 'Coin' give the number of errors and of successes. The column marked 'Average' gives the average of all determinations without regard for sign; the next column is the mean variation of this average. Columns headed 'Pos. Average,' and 'Neg. Average' give the average error for the positive and negative errors respectively. The final column is the difference between the sum of the positive and the sum of the negative errors, divided by the total number of determinations, together with the sign of the preponderating error. All results are given in degrees. Numbers in brackets are estimated.

TABLE I

SUBJECT III

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1	14	5	0	6.74	3.59	7.50	4.60	+4.31
2	15	4	2	3.05	1.87	3.60	2.50	+2.09
3	16	3	2	3.14	2.10	3.43	3.66	+2.09
4	12	2	8	1.91	1.73	2.83	4.00	+1.18
5	18	3	0	2.90	1.70	3.23	1.00	+2.62
6	15	3	3	2.14	1.23	2.53	2.33	+1.47
7	9	4	9	1.45	1.31	2.11	3.25	+0.27
8	14	5	2	3.04	1.88	2.71	5.20	+0.57
9	8	11	3	5.02	2.10	7.28	5.40	−0.18
Total...	121	40	29	3.18	2.10	3.91	2.45	+1.46

In series '5,' O. remarked that he never went below the click, if he could help it, because the 'after whirl' bothered him. About the middle of series '6,' the experimenter asked O. if he were satisfying himself that the points selected were actual coincidences. O. thought that he was staying a little positive. 'If he went farther he was bothered by the after-image streak.' At no time did the experimenter discuss the point; O. was merely urged from this time to make his determinations as accurate as possible. In series '8' and '9' the complete half circle was first used. Previous to this, determinations were in the left upper quadrant.

After three days' practice, in which subject I. was allowed to know the type of error she had been making and to study

a few cases of actual coincidence, she tried again. The results this time were but slightly better than before. This subject was familiar with the discussions of theory and was particularly interested in the various theories of apperception,

TABLE II

SUBJECT I

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1	10	10	1	10.71	6.43	9.60	12.90	− 1.57
2	9	9	3	6.52	5.17	5.00	10.22	− 2.23
3	14	5	2	7.52	4.78	10.71	3.40	+ 5.90
4	19	1	0	13.90	6.42	14.47	3.00	+13.57
5	17	1	1	12.00	6.63	12.76	11.00	+10.84
6	18	1	1	10.40	5.06	11.22	6.00	+ 9.80
7	20	1	0	16.85	8.16	17.65	1.00	+16.85
8	13	4	3	8.30	4.53	10.38	7.75	+ 5.20
9	16	3	2	9.09	5.45	11.12	4.25	+ 7.85
10	19	1	1	13.71	7.19	13.78	6.00	+13.14
Total...	155	36	14	10.92	5.98	12.60	8.6	+ 7.91

etc., offered to explain the source of error. Her introspections clung to questions of priority of sound or of pointer in attention, the 'pulling' influence of the sound in the negative direction, and the tendency of the eyes to follow the pointer. If at any time during the 'practice with knowledge' work she was urged to go on to where the sound 'pulled' her, she found a point that was always more correct than the first point selected. During the remainder of this phase of the practice and the work of the table below, she developed a habit of finding this first assumed point; stopping a moment, then moving to a second satisfactory determination. If she went where the sound seemed to pull her, the results were always better than otherwise. Subject I. failed decisively in finding that the pointer was a part of consciousness of position at all. She would regularly reply to questions intended to elicit descriptions of the pointer, etc., 'Of course I know where it is but I don't see it; the *sound* is at the place I select finally.'

We now made a change in the directions to O. Subject I. was asked to find, by the usual method, the place on the

dial where her vision of the dial was *interrupted* by the pointer *when* the click sounded. The results follow in Table III.

TABLE III

SUBJECT I. TO FIND 'INTERRUPTION'

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
19	9	10	2	3.64	2.23	4.22	3.80	0.00
20	6	11	1	3.83	3.09	3.16	4.54	−1.72
21	5	3	9	1.30	1.40	2.80	2.66	+0.35
22	4	3	3	2.70	2.10	4.00	3.66	+0.50
Total...	24	27	15	2.86	[2.20]	3.54	3.66	−0.31

Subject I. still felt the 'pull' and the 'push' of sound and of pointer, but she was now looking for a *definite visual occurrence* at the time of the click and succeeded in eliminating the effects of the previous adjustment to a large extent. At this point, however, no introspections from 'O.' enabled us to find the reasons for such steadiness of the positive error in the previous experiments.

TABLE IV

SUBJECT II

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1	7	14	0	8.09	2.88	7.71	8.28	− 2.95
2	5	15	1	6.85	3.67	8.00	6.93	− 3.05
3	5	15	0	14.40	6.85	12.60	15.00	− 8.10
4	5	13	3	6.04	3.48	4.80	7.92	− 3.61
5	8	11	1	6.35	3.82	6.00	7.18	− 1.55
6	5	14	2	8.61	6.26	2.20	12.17	− 7.51
7	2	18	1	7.14	3.71	2.00	8.11	− 6.76
8	0	19	1	10.15	4.48	.....	10.68	−10.15
9	0	19	1	12.80	6.20	.....	13.47	−12.80
10	5	12	4	7.33	5.61	5.80	10.50	− 4.57
Total...	42	150	14	8.73	4.79	5.6	10.2	− 6.13

Subject II. was familiar with the experiment. He knew the results of the various methods of observation, yet here we see him passing over all the other possible 'cues,' to what must be the final adjustment among all. The effort most

prominent in O.'s mind was fixation; together with this was attention on the pointer except when the sound failed to coalesce with pointer. The important point in introspection was the difficulty connected with the long spread of the pointer at these rapid rates.

At the close of this series, three of the observers attempted to examine the peripheral, or objective factors more closely. All observers were allowed to know the direction of their error. We may quote certain typical statements from the laboratory notes of subject II. These are from the practice series with knowledge.

Coincidence is at 328; O.'s final determination, 327; tentative fixation points, 360, 340, 330, 335, 325, 327. O. reports: "Trouble in seeing the pointer; can't distinguish between pointer and black streak, yet have a distinct feeling that there is a black streak below 320,—no streak above 330." Several days later, O. wrote: "Felt able to distinguish the pointer disturbing fixation, from the positive after image,—it is a difficult task to combine pointer and click even when the pointer is seen clearly." On this day, all determinations, after the first, were either positive or correct; the first was negative three degrees.

The time relation is now the problem presenting the most difficulty for this subject. O. felt that he could seldom satisfactorily combine the two elements into one percept. There was always either a space or a distinct sense of time between. After the difficulty was noticed a few times as a temporal judgment, this seemed to disappear, or, in the later trials, to dissolve into the space relation. When pointer and click are held apart by the space that separates them it is almost impossible to determine which position is the point of coincidence. Sometimes actual coincidence proves to be just between the two points selected; sometimes it is a few degrees below the lower one; more often, with subject II., actual coincidence is above either of the assumed positions. If actual coincidence is 320, the two separate assumed coincidences may be 317 and 310. In this case, such conditions seem to be explained by the click in relation to one or the

other end of the 'blur' or near the vanishing point of the positive after image.

Subject IV. shows the more usual distribution of equal numbers of positive and negative errors. (In an earlier series, he had been regularly positive.) In this group of determinations, Subject IV. made shifts in fixation that amounted to approach from both directions. Toward the close, the experimenter also discovered that O. was not attempting to make determinations more accurate than the five-degree divisions. Both conditions operate to produce the equal distribution of positive and negative errors.

Series '7' to '12' inclusive were spent in studying the factors involved, so far as the subject could introspectively ascertain them, as he made the determinations. In series '12' a black screen with white scale marks and the pointer with white tip were used.

TABLE V

SUBJECT IV

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1	3	14	2	4.94	3.52	5.00	5.64	−3.37
2	3	13	4	3.85	2.73	2.00	5.46	−3.25
3	13	5	2	4.75	2.87	6.00	3.40	+3.05
4	8	8	4	4.10	2.91	5.75	4.50	−0.50
5	7	10	4	2.43	1.58	2.43	3.40	−0.81
6	14	6	1	5.09	2.73	5.00	6.16	+1.57
Total...	48	56	17	4.18	[2.72]	4.64	5.07	−0.59
7	2	2	1	5.00	1.80	5.50	7.00	−0.60
8	7	1	3	2.45	1.59	3.57	2.00	+2.09
9	7	3	2	2.50	1.33	2.86	3.33	+0.83
10	0	6	3	1.22	0.86	.....	1.83	−1.22
11	5	2	1	3.35	1.62	3.00	6.00	+0.35
12	4	5	0	2.88	1.65	3.75	2.20	+0.44
Total...	25	19	10	2.70	[1.47]	3.44	3.15	+0.48

Subject IV. found difficulty with the after image from the beginning. Often a fixation point would be selected that seemed confused with the image, and O. would change to a safer position. The change in these instances increased the error to a more positive one, if O. had been proceeding

cautiously. O.'s criterion was 'to make the pointer slap the mark'; it would seem to bend if he felt he had reached a negative position. Sometimes he attempted to select the 'blackest' part of the streak; in other instances, the best position was where 'the scale seemed to brighten up.' The 'streak' that subject IV. is attempting to analyze was often 20 degrees in length. More commonly it was from five to ten degrees shorter.

Subjects III. and IV. report this disturbing influence of the after image constantly. Subject I. makes no report on it; she is concerned with the 'pull' that the sound has. Subject II. was still uncertain whether he had been led to make negative errors through attention to the sound,[1] or because of the failure to distinguish between the after image and the 'blur' of the pointer, or possibly because of some unnoticed movement of the eyes downward.

In the practice experiments, I (subject II.) gave special attention to the difference between after image and pointer. At the same time I attempted to get the most advanced moment of interruption. The results are massed together in Table VI. below. In series 21, I used the black screen and pointer with white tip.

As can be seen, I succeeded in eliminating the preponderance of negative errors fairly well, and in reducing the general average (see Table IV. above). My tendency proved to be one that led me to select the scale position that was free of the pointer at the time of click. (Prior entry of sound?) The tendency was eliminated by closer attention, not to my fixation, but to the *features of the dial at the fixation point*, so that I might recognize the *first* change or 'interruption' of vision due to the passage of the pointer. Under these conditions the blur can seldom be seen. To my astonishment, the sound still did not seem to occur at this first 'interruption' point, it continued to stand at the 'scale position that was free of the pointer'; caught, of course, with peripheral vision at a later revolution of the pointer. Experience showed me, however, that if I did take this advanced

[1] Titchener, E. B., 'The Psychology of Feeling and Attention,' p. 253 f.

point,[1] I would be more accurate than by using the other methods. As a rule, blur, after images, angles of the pointer, and other features of the dial are eliminated by the successful attainment of this form of fixation. Squinting, closing one eye, etc., aid at times in this elimination. There is, naturally, no peripheral vision, or a very limited field, during this manner of fixation, hence the procedure becomes one of alternation between 'fixation with peripheral vision' and 'fixation to catch the visual interruption.' The black dial with white scale marks, and a white tipped pointer, give the most easily seen 'interruption,' so far found. We have not yet had time to test this thoroughly.

TABLE VI

SUBJECT II

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
11 to 20.	58	72	23	5.03	.....	5.82	6.01	−0.62
21.....	8	11	4	2.21	1.84	3.87	1.91	+0.43

We have quite accidentally found four subjects whose results fall in separate classes. Subject I. makes the positive error. Subject II. emphasizes the negative error. Subject III. is positive, but his error is so small that it perhaps comes within the limit of accuracy set by the scale marks, or that set by the daily disposition and accidental combinations of attention. The error made by subject IV. is slightly larger, but the distribution is nearly even. All four were using the same apparatus and presumably following the same set of directions. After studying the features of the moving pointer as they appear on the dial, and then placing emphasis in the directions on a point overlooked previously, the visual interruption, the results of all four subjects' determinations are brought into closer correspondence.

It seems evident from the above material, and other work since Geiger's article is corroborative, that such factors as eye movements, after images, the 'blur' of the pointer, the

[1] See Dunlap, *op. cit.*, p. 168.

ease or difficulty experienced in finding the 'pointer interruption' position, and other positional factors, such as 'Umkehr' points, angles made by the pointer (as it changes direction across the field of vision), etc., are essential elements in irregularities of error of the grosser sort. With this method also, to attain subjective satisfaction is always a matter of great difficulty. It can be kept only for very short periods of time, and is largely a matter of preference. The usual judgment is: "This is the best I have found; might find a better if I watched longer."

The existence of a temporal 'indifference-zone' seems fairly certain. Under usual conditions the 'blur' and after image streak are distinctly noticeable. At the ends of, or within, this region of the streak, the 'indifference-zone' may vary without conscious change. The assumed coincidence will depend on the momentarily important factors, and any theory O. has concerning actual coincidence within this constellation of elements. The larger errors ought to show a regular distribution around some point other than the actual coincidence, indicating that one of the factors is exercising a primary influence in the judgments. The latter result appears especially well in our experiments.

III

All of our observers, except subject I., mentioned the uneasiness attending determinations, on account of the after image. We, therefore, attempted to plan an arrangement of apparatus that would either eliminate this factor or at least reduce its effect on the observations.

We used in place of the pointer a revolving disk that covered the inner surface of the dial. This disk barely touched the inner ends of the scale marks. It had on the circumference, at the edge, a black spot, approximately the size of the black tip of the pointer. The actual size of the spot was 8 mm. along the radius of the disk and 5 mm. wide. Such an arrangement still leaves the blur of the moving spot,[1] and, as experiment shows, if fixation shifts to the scale marks,

[1] See Dunlap, *op. cit.*, p. 179.

often gives an after image. Sometimes it appears as long as that produced by the pointer, but usually the short 'blur' is all that can be noticed.

TABLE VII

(*A*) DISK

Subject V

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*b*	6	3	1	5.3	2.96	5.16	7.33	+0.80
2*a*	8	2	0	3.8	2.16	3.75	4.00	+2.20
3*b*	2	8	0	4.6	1.6	5.0	4.50	−2.60
4*a*	6	2	2	4.8	3.3	6.83	3.50	+3.40
5*b*	1	6	3	3.9	3.68	2.0	6.16	−3.50
6*a*	2	8	0	3.6	1.68	3.5	3.62	−2.20
7*b*	2	7	1	2.8	1.36	1.5	3.57	−2.20
Total...	27	36	7	4.1	2.5	4.55	4.55	−0.37

(*B*) POINTER

1*a*	0	10	0	12.5	6.8	.....	12.50	−12.5
2*b*	1	8	1	3.9	2.54	3.0	4.50	− 3.6
3*a*	0	10	0	9.1	2.1	.....	9.10	− 9.1
4*b*	1	9	0	7.2	3.64	2.0	7.77	− 6.8
5*a*	0	10	0	10.9	3.1	.....	10.9	−10.9
6*b*	1	9	0	9.8	5.0	1.0	9.6	− 9.6
7*a*	0	10	0	15.1	4.48	.....	15.1	−15.1
Total...	3	66	1	10.07	4.95	2.33	10.57	− 9.87

(*C*) DISK

8*a*	8	1	1	2.9	1.70	3.37	2.0	+2.5
9*b*	9	1	0	7.4	4.18	8.00	2.0	+7.0
10*a*	9	0	1	4.6	2.92	5.11	.....	+4.6
11*b*	7	2	1	4.5	2.6	5.28	4.0	+2.9
12*a*	7	3	0	5.2	1.44	5.42	4.6	+2.4
Total...	40	7	3	4.86	2.57	5.42	3.7	+3.82

(*D*) POINTER

8*b*	5	4	1	2.5	1.1	2.8	2.7	+0.3
9*a*	6	3	1	7.5	4.1	6.5	12.0	+0.3
10*b*	9	1	0	6.4	2.7	6.4	6.0	+5.2
11*a*	8	1	1	4.0	2.2	4.5	4.0	+3.2
12*b*	2	7	1	3.9	2.3	2.0	7.0	−3.1
Total...	30	16	4	4.82	3.0	4.86	5.7	+1.44

ease or difficulty experienced in finding the 'pointer interruption' position, and other positional factors, such as 'Umkehr' points, angles made by the pointer (as it changes direction across the field of vision), etc., are essential elements in irregularities of error of the grosser sort. With this method also, to attain subjective satisfaction is always a matter of great difficulty. It can be kept only for very short periods of time, and is largely a matter of preference. The usual judgment is: "This is the best I have found; might find a better if I watched longer."

The existence of a temporal 'indifference-zone' seems fairly certain. Under usual conditions the 'blur' and after image streak are distinctly noticeable. At the ends of, or within, this region of the streak, the 'indifference-zone' may vary without conscious change. The assumed coincidence will depend on the momentarily important factors, and any theory O. has concerning actual coincidence within this constellation of elements. The larger errors ought to show a regular distribution around some point other than the actual coincidence, indicating that one of the factors is exercising a primary influence in the judgments. The latter result appears especially well in our experiments.

III

All of our observers, except subject I., mentioned the uneasiness attending determinations, on account of the after image. We, therefore, attempted to plan an arrangement of apparatus that would either eliminate this factor or at least reduce its effect on the observations.

We used in place of the pointer a revolving disk that covered the inner surface of the dial. This disk barely touched the inner ends of the scale marks. It had on the circumference, at the edge, a black spot, approximately the size of the black tip of the pointer. The actual size of the spot was 8 mm. along the radius of the disk and 5 mm. wide. Such an arrangement still leaves the blur of the moving spot,[1] and, as experiment shows, if fixation shifts to the scale marks,

[1] See Dunlap, *op. cit.*, p. 170.

often gives an after image. Sometimes it appears as long as that produced by the pointer, but usually the short 'blur' is all that can be noticed.

TABLE VII

(*A*) DISK

Subject V

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*b*	6	3	1	5.3	2.96	5.16	7.33	+0.80
2*a*	8	2	0	3.8	2.16	3.75	4.00	+2.20
3*b*	2	8	0	4.6	1.6	5.0	4.50	−2.60
4*a*	6	2	2	4.8	3.3	6.83	3.50	+3.40
5*b*	1	6	3	3.9	3.68	2.0	6.16	−3.50
6*a*	2	8	0	3.6	1.68	3.5	3.62	−2.20
7*b*	2	7	1	2.8	1.56	1.5	3.57	−2.20
Total...	27	36	7	4.1	2.5	4.55	4.55	−0.57

(*B*) POINTER

1*a*	0	10	0	12.5	6.8	.....	12.50	−12.5
2*b*	1	8	1	3.9	2.54	3.0	4.50	− 3.6
3*a*	0	10	0	9.1	2.1	.....	9.10	− 9.1
4*b*	1	9	0	7.2	3.64	2.0	7.77	− 6.8
5*a*	0	10	0	10.9	3.1	.....	10.9	−10.9
6*b*	1	9	0	9.8	5.0	1.0	9.6	− 9.6
7*a*	0	10	0	15.1	4.48	.....	15.1	−15.1
Total...	3	66	1	10.07	4.95	2.33	10.57	− 9.87

(*C*) DISK

8*a*	8	1	1	2.9	1.70	3.37	2.0	+2.5
9*b*	9	1	0	7.4	4.18	8.00	2.0	+7.0
10*a*	9	0	1	4.6	2.92	5.11	.....	+4.6
11*b*	7	2	1	4.5	2.6	5.28	4.0	+2.9
12*a*	7	3	0	5.2	1.44	5.42	4.6	+2.4
Total...	40	7	3	4.86	2.57	5.42	3.7	+3.82

(*D*) POINTER

8*b*	5	4	1	2.5	1.1	2.8	2.7	+0.3
9*a*	6	3	1	7.5	4.1	6.5	12.0	+0.3
10*b*	9	1	0	6.4	2.7	6.4	6.0	+5.2
11*a*	8	1	1	4.0	2.2	4.5	4.0	+3.2
12*b*	2	7	1	3.9	2.3	2.0	7.0	−3.1
Total...	30	16	4	4.82	3.0	4.96	5.7	+1.14

Directions to the observers remained as before. New subjects were not cautioned concerning the possibility of after images, of the use of an 'interruption' point, etc. Subjects I. and II. of the previous group acted as subjects here. Subjects V., VI., and VII. were new. None of the three knew the purpose of the experiment and only one had seen the apparatus before. Twenty determinations were taken a day. Ten of the twenty were with the pointer and ten with the disk. If we began with the pointer one day, the next began with the disk. The entire left half of the dial was used. Actual coincidence was given in haphazard order. The arrangement of tables is the same as above.

Table VII. represents the first work subject V. had with the apparatus. He was unacquainted with the previous theories and developed his own statements of method. During the first day's work, O. remarked: "When I started I put coincidences in the middle of the long streak; now I am putting them at the lower point, because it is the click that calls my attention to the place and the impulse to follow the pointer would carry me beyond." O. studied the possibility that the 'shadow' might be behind the pointer, but decided that this was not the case here—he could see the pointer up to the click. In series '3,' O. explains his theory still further. "I push around to the lower end of the image, since I seem to 'spot' the click all right, and the after *image spreads out afterward*," *i. e.*, after the click is heard, and as a consequence of the continued movement of the pointer. His theory is not so applicable to the spot on the moving disk, but O. fails to think of this.

O. continued using this procedure until the eighth day. The disk came first this time. O. reported that the disk seemed much easier. The 'spot' on the disk was more easily seen than the black tip of the pointer. Characteristic remarks in series '8' and '9' are: "Didn't notice the after image today:" "Simply tried to pick out where it (coincidence) was and called it." "It depends on where you fixate where the streak appears." O. was not certain whether he had not been using something like this with the disk up to this time. The pointer now hits out the sound for O.

The difference between results with pointer and those with disk is striking. Even with the negative influence of the pointer (which O. consciously used in the first seven) the determinations with the disk are easily within the 'indifference-zone.' 66% of all errors are smaller than the general average, which is 4.1. On the other hand the errors with the pointer are almost all negative and 56% of the errors are smaller than the general average, 10.07. After O. changed his theory and sought to find the place 'where the pointer or spot on the disk hit out the sound,' the difference becomes less. Practically the same factors are now operative in both forms of the apparatus. With either theory the results with the revolving disk are within the temporal 'indifference-zone.' It is possible that with such a large field

TABLE VIII

(*A*) DISK

Subject I

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*a*	5	4	1	4.6	3.62	3.2	7.5	− 1.4
2*b*	5	5	0	6.7	4.24	6.8	6.6	+ 0.1
3*a*	4	5	1	6.4	3.80	7.0	7.2	− 0.8
4*b*	10	0	0	10.6	5.32	10.6	.....	+10.6*
5*a*	2	8	0	5.9	2.08	7.0	5.6	− 3.1
6*b*	3	6	1	4.8	3.00	6.6	4.6	− 0.8
Total...	29	28	3	6.5	3.91	7.5	6.1	+ 0.76

(*B*) POINTER

1*b*	7	3	0	9.0	5.8	10.14	6.3	+ 5.2
2*a*	5	5	0	6.3	3.5	5.60	7.0	− 0.7
3*b*	8	2	0	7.0	4.4	7.87	3.5	+ 5.6
4*a*	9	1	0	6.3	2.8	6.55	4.0	+ 5.5
5*b*	5	5	0	7.8	3.4	10.00	5.6	+ 2.2
6*a*	5	4	1	7.6	5.0	9.40	7.2	+ 1.8
Total...	39	20	1	7.33	4.17	8.15	6.1	+ 3.26

revolving before the eyes, they will be pulled in the direction of the movement of this field. To avoid this, the fixation point with disk always included a definite mark on the dial, causing frequent after images. Further, O. never fails to report a blur of the spot on the revolving disk. The distinct-

ness with which the spot can be made to stand out clearly by the least eye movement, makes this differential easy for introspection.

Table VIII. is the work of subject I. It will be remembered that this observer regularly made positive errors in the earlier experiments and found it very difficult to overcome the tendency. She had not worked with the apparatus for six weeks. With the disk she makes a set of determinations that are equally positive and negative. The positive tendency still remains in the determinations made with the pointer. It appeared once in the use of the disk, series '4b' (* in table). My notes here say: "It seems absolutely impossible for O. to get any results that are satisfactory to her with the disk today." The notation 'lost' appears five times in the series of ten, and O. could not work into her disk method. The pointer procedure, which also was unusually positive today, persisted with the disk. The determinations with the pointer seemed fair to O., but with the disk she did not obtain subjective satisfaction in any case. The 'indifference-zone' is relatively steady.

TABLE IX

(*A*) DISK

Subject II

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*a*	1	9	0	8.6	3.88	2.0	9.33	− 8.2
2*b*	1	9	0	6.3	3.62	6.0	6.33	− 5.1
3*a*	3	7	0	7.5	4.40	4.3	8.71	− 4.7
4*b*	2	7	1	5.5	4.10	3.5	6.85	− 4.1
5*a*	0	10	0	9.5	4.80	.....	9.50	− 9.0
6*b*	4	6	0	8.2	5.04	9.2	7.50	− 0.8*
Total...	11	48	1	7.6	4.47	6.0	8.12	− 5.4

(*B*) POINTER

1*b*	3	5	2	2.9	1.92	4.33	3.2	− 0.3
2*a*	4	6	0	6.3	5.13	6.00	6.5	− 1.5
3*b*	9	1	0	12.4	5.00	13.22	5.0	+11.4
4*a*	9	1	0	9.0	5.60	9.77	2.0	+ 8.6
5*b*	9	1	0	7.8	5.72	8.55	1.0	+ 7.6
6*a*	8	2	0	7.7	4.04	8.50	4.5	+ 5.9
Total...	42	16	2	7.68	5.32	9.26	4.5	+ 5.28

In the middle of '6*b*' (* in table) with the disk, O. accidentally saw that the last was a few degrees negative. This threw him off entirely in the remainder of the series. The results of Table IX. are poorer than usual since O. had spent the week just preceding with the oculist, and his eyes were not yet in shape to maintain the necessary fixation.

Subject II., the writer, was regularly negative in the earlier experiments. When it came to the use of the pointer in this experiment, O. endeavored to catch the *first* 'interruption point.' Since this was the first work with the disk, there was no way of knowing how to use it. The main difficulty noted by introspection, was the inability to close the space between pointer and click. With the disk, O. found it easier to select the 'interruption-nothing' sequence; while with the pointer, he sought continually to avoid the after image. As results indicate, both attempts were successful. However, the failure to close the 'space' gap reversed the form of the errors. With this 'time' or 'space' gap unclosed, certainty in the judgment of visual 'interruption' tends to produce positive errors with the pointer. Especially is this true if O. is consciously avoiding the after image and as our procedure was arranged, approaching actual coincidence from the positive error side. Introspectively, this 'space' gap with the disk, was clearest when pointer and click were together and this was followed by a blank in the field of vision. This is the 'interruption-nothing' sequence. This proved to be the negative error.

If we examine the individual determinations in this set of records, we find that there is a very irregular distribution curve. With the pointer, twelve of the sixty are errors of 16 degrees and above. With the disk, the error distribution is regular, though the number of determinations is small. The distribution in O.'s previous work was regular, indicating that unfavorable conditions existed here.

In the time available, subject VII., Table XI., did not succeed in obtaining regularity of control. The records still show the differential effects of the disk and pointer. For example, if we compare the results of the first five series

TABLE X

(*A*) DISK

Subject VI

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*b*	1	9	0	6.4	4.28	1.0	7.00	− 6.2
2*a*	0	10	0	17.7	4.60	.....	17.70	−17.7
3*b*	0	9	1	17.6	8.60	.....	19.55	−17.6
4*a*	1	9	0	19.0	7.20	1.0	21.00	−18.8
5*b*	0	9	1	10.2	7.28	.....	11.33	−10.2
6*a*	0	10	0	12.3	5.64	.....	12.30	−12.3
Total...	2	56	2	13.86	7.47	1.0	14.82	−13.8

(*B*) POINTER

1*a*	1	7	2	5.8	4.6	2.0	8.0	− 5.4
2*b*	0	10	0	25.7	5.5	.....	25.7	−25.7
3*a*	0	10	0	25.0	5.8	.....	25.0	−25.0
4*b*	0	10	0	10.3	6.4	.....	10.3	−10.3
5*a*	0	10	0	23.8	8.8	.....	23.8	−23.8
6*b*	0	8	2	10.1	6.7	.....	12.6	−10.1
Total...	1	55	4	16.78	8.93	2.0	18.27	−16.71

TABLE XI

(*A*) DISK

Subject VII

Series	Number of			Average	M. V.	Pos. Average	Neg. Average	Abs. Average
	Pos.	Neg.	Coin					
1*b*	9	1	0	34.0	13.4	37.6	1.0	+33.8
2*a*	4	6	0	16.9	6.5	16.0	17.5	− 4.1
3*b*	7	3	0	7.3	3.9	8.4	4.6	+ 4.5
4*a*	6	2	2	10.7	8.8	14.0	11.5	+ 5.9
5*b*	4	4	2	5.9	3.9	5.5	9.2	− 1.5
6*a*	2	8	0	8.1	4.7	10.0	7.5	− 4.0
Total...	32	24	4	13.78	10.2	18.54	10.0	+ 5.78

(*B*) POINTER

1*a*	9	1	0	25.9	11.48	27.55	11.00	+23.7
2*b*	6	3	1	7.5	3.6	8.33	8.33	+ 2.5
3*a*	7	2	1	7.6	5.4	9.14	6.00	+ 5.2
4*b*	8	2	0	9.0	6.8	9.75	6.00	+ 6.6
5*a*	5	4	1	6.2	3.6	9.00	4.25	+ 2.8
6*b*	0	9	1	15.2	7.4	.....	16.87	−15.2
Total...	35	21	4	11.9	8.17	13.86	10.9	+ 4.26

with the pointer, with the last five of the disk, we obtain for the pointer group a general average of 11.24 degrees and for the disk group, one of 8.15 degrees. Corresponding absolute averages are + 8.16 and + 0.18.

Subject VI. (Table X.) presents an interesting set of determinations, since he obviously used the 'simple reaction' method in the major portion of his judgments. The average falls on both the median and the mode, and he is consistently negative throughout. His procedure was to move from the first fixation point to others while listening for the sound. When he reached a point where he heard sound and *then* saw pointer, he stopped. The effect of the sound and pointer rhythms, as Dunlap has shown,[1] is to make this interval shorter than the normal 'simple reaction' time. Subject III. also used the sound rhythm, but he *saw* the pointer and click together. His characteristic method was to press the thumb and finger together at the click.

In Subject VI.'s results, we may still note the effect of the disk in removing the, in this case, negative influence of the after image.

The 'indifference-zone' remains a distinct and relatively important element. The varying prominence of adventitious conditions and of other factors noted by previous investigators, is also indicated. The full significance of each element cannot yet be made out. Otherwise the differential introduced by pointer *vs.* disk runs through the experiments here presented. The clear-cut way in which the determinations vary between disk and pointer and finally reverse as Subject V. changes his theory, is still demonstrable in the records of the other subjects, although complicating conditions are more numerous. We can almost measure the 'indifference-zone' in the average of the general averages of Table VII. *A*, *C*, and *D*. It is tempting to offer the difference (5.48) between this and 10.07, the general average for the pointer, given in Table VII. B., as the measure of the influence of the after image.

[1] Dunlap, *op. cit.*, p. 172 f.

IV

In the practice series, the regularity of results found by Burrow is again demonstrated. The importance of 'exact fixation,' emphasized by Dunlap, is, we believe, thoroughly corroborated by our data. The after image reappears as a factor in the determinations. The *visual* mental content is complex. In our preliminary experiments, we find evidence for nearly all of the factors mentioned by other investigators. Not all of our subjects make negative errors with the slow rates; nor did we succeed in producing consistent negative or positive errors with attention on sound or pointer respectively.[1]

What are some of the factors *seen* at the time of the click, or near the moment of judging coincidence? With the rapid rate used in these experiments, several phenomena may be noted. These vary with the locus of fixation. In following the pointer accurately, a spot that is clearly defined in shape, etc., is seen.[2] In 'exact fixation,' two elements are usually present, the 'blur' and the positive after image. Both depend on the rate of the moving pointer for their length and upon the illumination for their intensity. To obtain the blur no movement of the eye must take place. The appearance of the after image depends on the direction of attention. Unless one is looking for it, consciousness will not take account of its position and size. Our directions to the observer necessitate a distribution of attention over an arc of varying length; and, within the general field of vision, portions of this arc are seen in direct vision, while other parts appear only in peripheral vision. Along this arc, the blur will maintain a regular position at the point of fixation, and the after image will appear indifferently on either side of this point. Attention will note parts of this image.

With very slight eye movements this range is increased enormously. The distance from the point of fixation to which the 'pulses of sensation' extend and the number of their repetitions, seem to depend on the rate of the moving

[1] Stevens, *op. cit.*

[2] Dunlap, *op. cit.*, p. 181 f.

stimulus and its intensity.[1] Our observers speak frequently of seeing the pointer near the center of the entire disk. The change in angle it makes from the vertical at the lowest point to vertical again is confusing. Subjects trained to make determinations in the upper left quadrant immediately made greater errors when the lower left was added.

A third phenomenon is observed, when, in addition to 'exact fixation,' the subject attempts to limit attention to the 'interruption' of his vision of some seen object at the point of fixation, *e. g.*, the inner end of the scale mark, a defect in the cardboard material of the dial, etc. The tip of the pointer passing 'interrupts' this view. This seems a probable interpretation of the effects obtained by Dunlap in using a white mosquito netting to maintain fixation. Dunlap thus distinguishes by implication between the 'blur' and the 'after image.' He has called our attention to the use of this in detecting eye movements.[2]

Geiger undoubtedly has in mind the entire complex in his attempt to measure the influence of eye movements on the quality of the error.[3] Geiger finds that the greater length of the after image accompanies the upward movement of the pointer, and this condition indicates a greater eye-strain, which is favorable to more negative or less positive amounts in the error on the ascending side. Dunlap speaks of an after image, other than the 'blur,' only in connection with the 'pointer pursuit' method.[4] In our experiments, the 'interruption' was first consciously used by subjects III. and IV. I noticed subject III. squinting and frequently using one eye in making his determinations.[5] When questioned, he replied that he could see the place where the pointer hits the mark better. Subject IV. described it as 'slapping the mark.' It was through an attempt to verify these statements introspectively, that I discovered the great discrepancy between

[1] McDougall, W., 'The Sensations Excited by a Momentary Stimulation of the Eye,' *Brit. Jour. of Psychol.*, Vol. 1, p. 87 f.

[2] *Op. cit.*, pp. 158 and 168.

[3] *Op. cit.*, S. 426.

[4] *Op. cit.*, p. 184.

[5] Dunlap, *op. cit.*, p. 168.

my subjectively satisfactory determinations and the 'feeling' that actual coincidence gives (p. 108). The use of this factor with our method, reverses Burrow's procedure. He literally moved coincidence to the stationary mark, while here a series of stationary marks were tested and rejected till 'satisfactory coincidence' is found. *Such a visual cue is no longer a moving stimulus.*

In this paper, we have emphasized certain visual factors that enter into the determinations of simultaneity of two disparate stimuli under the conditions set by Wundt's form of apparatus. It is true that these are objective, or mere conditioning elements, and not a part of the fundamental problem set the observer. However, they are a part of the complex constellation of factors and when the final perception is formed, must either be involved as analyzable elements of that complex, or as non-conscious conditions affecting it. In general, a number of factors will be given each time, and the mind has set for it the problem of combining these in the most satisfactory manner. What that final perception will be depends on the factor or factors that are most prominent during that particular judgment.

All the factors that different observers have discovered may not be operative in the formation of any single perception; under the usual conditions of the experiment there are always several present. These are to be united into a perception that has for its central unity the sound and the pointer. Certain main variations have been worked out. For example, the sound 'floats' just as soon as the subject relaxes his fixation. It floats, of course, in one or the other direction according to the prominence of some phase of the attention process. This attention process, I believe, is primarily conditioned by the changing and unstable relations that appear in the field of vision, such as we have mentioned above. The habit character of this perception is excellently illustrated thus: If one has practiced making determinations with a certain quadrant, change to another is immediately productive of uncertainty in the judgments and distinct increase in the error. The effect of different directions of approach also

illustrates the point. The experiment offers an especially difficult problem to perception.

Summary

1. Using a rigid method of observation, we see that practice tends to establish a regular distribution curve, which curve exhibits a mode in one or the other form of error. The inference that this is due to the observer's preference for some particular criterion, rather than to some new element in the attention process, seems warranted by the introspections.

2. Results with a revolving disk that tends to destroy the after image and localize the visual factors, differ materially from those with the usual form of rotating pointer. These differences seem closely connected with introspectively observable changes in the length and intensity of the after image.

3. With the 'interruption point' as the 'position' to be united with 'click,' subjective satisfaction is more difficult to obtain but when obtained, is much more accurate than when other criteria are used.

A COMPARISON OF THE FACTORS INVOLVED IN THE MAZE LEARNING OF HUMAN ADULTS AND CHILDREN

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In an account published a year ago, one of the writers of the present paper reported an investigation of the human learning process in the maze.[2] In a previous article, Hicks and Carr had attempted a correlation of the reactions of the white rat, the human adult and the child.[3] A comparison of the two papers suggested to us the desirability of additional investigation in the field of correlation, for the following reasons: The human mazes used in the two investigations were radically different, the one employed by Hicks and Carr being the more simple. Inasmuch as much emphasis was placed upon the fact, in the first report referred to, that the type of learning curve is conditioned largely by the pattern of the maze itself, we assumed that with a maze somewhat more difficult than that used by Hicks and Carr, different results might be found. Our assumption proved to be entirely legitimate, as the two sets of curves obtained present a considerable amount of dissimilarity. In the second place, it was hoped to obtain somewhat more extensive data than those reported in either of the papers. While results upon the additional data obtained were not all illuminating, several items were recorded that we think merit discussion, and some criticisms of the maze experiment suggested.

[1] The maze tests reported in this article were conducted jointly by both experimenters. The paper was written by Dr. Perrin, who assumes responsibility for the views herein contained.

[2] Perrin, F. A. C., 'An Experimental and Introspective Study of the Human Learning Process in the Maze,' *Psychological Monog.*, 1914, Vol. XVI, No. 70.

[3] Hicks, Vinnie C., and Carr, H. A., 'Human Reactions in a Maze,' *Jour. of Animal Behavior*, 1912, Vol. 2, No. 2.

A pencil maze was used in the present investigation. The pattern, drawn in correct proportions, with the numbers and letters used to designate the various segments, is given in Fig. 1. The grooves, constituting the paths, were of the uniform width of 1 centimeter; the area bounded by the paths 22–1, 3, K, and 19 was 46 by 30 centimeters. The true path measured 210.8 centimeters in its entire distance, and the cul-de-sac segments totaled a length 117.3 centimeters, thus making a total of 328.1.

The maze was constructed by cutting a sequence of grooves through a board 1 centimeter in thickness, and by fastening this with screws to a second board, of the same thickness and size, that served as a base and prevented warping. Between the two a sheet of zinc was inserted, to serve as a floor for the maze. The pencil or tracer was made of hard rubber, rounded at the end, with an aluminum straight handle. It was about 7 millimeters in diameter—so that when held in the proper position in the path, it was not necessarily in contact with either side of the passage.

We had two objects in mind in designing the maze. The main interest was to present a task of considerable, but not extreme, difficulty to the two sets of learners. This was accomplished (1) by a sufficient number of paths and cul-de-sacs—22 segments of the former, 10 of the latter; (2) by cul-de-sacs of varying complexity, and of somewhat different types; (3) by the element of similarity in maze formations that had proved to be a source of confusion and difficulty in our previous experiment. This third element in design also served to accomplish our second purpose, which was to afford opportunity for analyzing and discriminating, as well as for memorizing. We hoped, by the introduction of this situation, to obtain data on possible differences in mental adjustment made by the children and adults. The maze proved to be of about the degree of difficulty desired. The second consideration is discussed in detail in an ensuing section.

Fifteen children and about 25 adults learned the maze. Our results are based upon the records of 10 of the former and 14 of the latter. The ages of the children are given in

Table II. They averaged 11.6 years. They were, with two exceptions, brothers or sisters of students in the university, and we were careful to select those who seemed to be thoroughly normal. The adults with one exception were recruited from the undergraduate ranks of the university, and the exception, *Hv*, was a teacher in the public schools. The adults should average about 21 years of age. The distribution by sex was as follows: children, 4 girls and 6 boys; adults, 7 and 7.

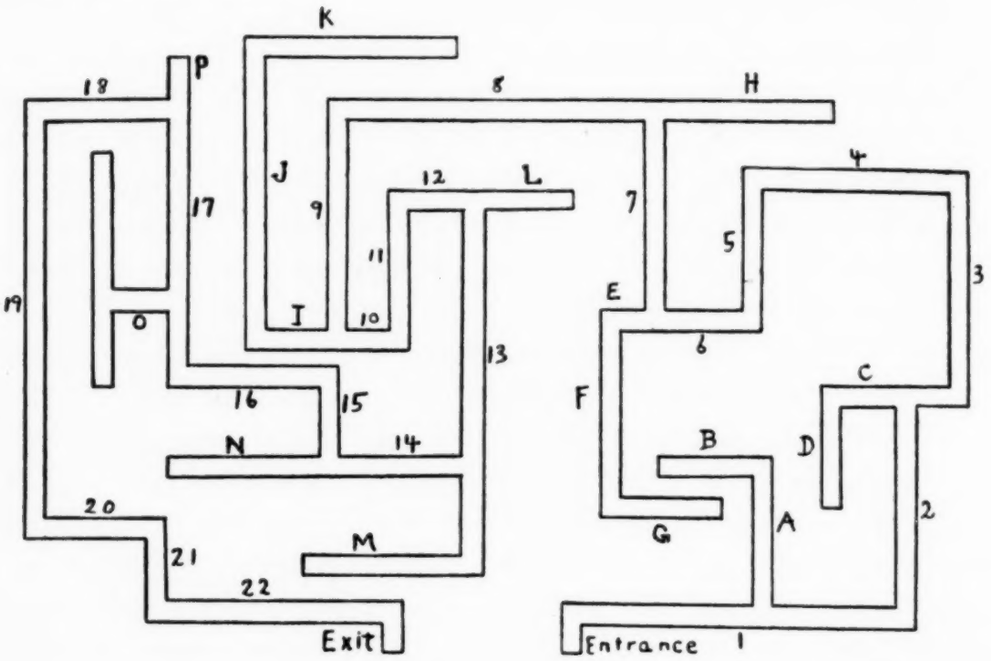

FIG. 1. Diagram of the Maze.

One child refused to apply himself to the task with what we considered to be a normal degree of interest, and never technically learned the maze. His record was accordingly discarded, as were the records of those who were subjected to some unavoidable irregularities during the course of the experiment. We knew the adult learners personally and we were careful to select them on the basis of normality. Both children and adults regarded the performance as a puzzle, and most of them, the children in particular, asked for more mazes at the end of the tests.

The trials were made in sequence. The subjects were

given all the time between trials they desired for rest, physical and mental recreation. We recorded, for each trial, (1) time, (2) number of errors, (3) complete route traversed. One experimenter recorded the route, while the other watched the time and checked off the errors. The learners were permitted and encouraged to make oral comments during the trials. These comments were found during the previous experiment not to be a distraction when they were made spontaneously. They were always interesting and expressed personal reactions more definitely than did their more formal replies to questions. In the questions, asked after each trial, they were instructed to tell us, "how they learned it; what places were difficult, and why; what segments they recognized," etc. Formal introspections on imagery were neither attempted nor desired.

In studying the results obtained, we made comparisons from as many different points of view as the data afforded, in the hope that at least certain aspects of the learning would disclose differences between the two groups and suggest correlations. The following represent the various aspects of the learning activity available: (1) quantitative results—totals and averages of number of trials, time, error and distance records, and rate; (2) the distribution throughout the trials of these measurements; (3) knowledge of the maze, at various stages of the learning, and the development of mental reaction to the maze.

I. The Maze Test and its Criteria of Efficiency

There have been, as yet, no satisfactory criteria established for measuring the ability of an animal or human learner in a maze. In fact, the different procedures followed by Yerkes, Watson, Carr and others, concerned not only with the kind of measurement to be recorded, but with the mathematical treatment of this measurement, have made comparisons impossible. The original controversy centered about the relative values of the time and error records; at Watson's laboratory, recent attention has been given to the path record. Vinnie C. Hicks discussed the problem in 1911 in

an interesting critical paper.[1] Her conclusion favored the time curve as the best single criterion; not, however, because of its theoretical importance alone, but because "it will yield the best results at the hands of different investigators."[2] Her final conclusion spoke for a combination curve, constructed from both time and error data.

The present writers are inclined to think very strongly that the diversity of opinion in the evaluation of different results is due to the absence of any accurate definition and criticism of the maze experiment itself, as it is ordinarily conducted. Carr emphasized the difficulty of interpreting the learning curve in general, in a sentence by no means conservative: "As yet practically nothing is known of the real meaning of a learning curve and the interrelations of its several features."[3] Certainly, a specific experiment, purporting to call into function any type of intelligence, should be analyzed rather carefully before its results consciously or unconsciously influence our theory of comparative intelligence. Three criticisms rather insistently presented themselves in our study of the results of this experiment, and while they have not, in our opinion, vitiated our conclusions, they have made the reservations which we have attached to them seem advisable.

In the first place, we believe we are justified in raising the question of whether the maze is a fair test of any type of learning ability, inasmuch as it places a rather definite premium upon purely chance discovery. Then, we are not convinced that the different experimenters have emphasized or even recorded all of the significant data. Lastly, we find in the results of the experiment under discussion a noticeable absence of correlation of the various records made by the same individual. These differences must be explained before the respective records are evaluated.

1. Without question, the factor of accidental discovery of correct turns plays an important rôle in the learning of a

[1] Hicks, Vinnie C., 'The Relative Value of the Different Curves of Learning,' *Jour. of Animal Behavior*, 1911, Vol. 1, No. 2.

[2] Hicks, *op. cit.*, p. 156.

[3] Hicks and Carr, *op. cit.*, p. 123.

maze, at least, with human beings. This is, of course, assumed to be one of the conditions of the experiment. But, unlike most human learning problems, the possibility of logical anticipations of most probable solutions is precluded, during the traversing of any passage for the first time. As the learning process of the human in the maze involves conscious analysis, it is to a certain extent a matter of pure luck whether either *A* or *B*, two subjects equally well equipped, is going to have the better record. This is the frequent testimony of the subjects themselves, and their scores seem to give some credence to their claim. One may not necessarily argue the point from a comparison of the first trial with the ensuing trials. As a matter of fact, the values for the initial trials average practically the same for both the good and the bad learners. Chance may operate during the later stages of the learning: *A* and *B* may both, through a slip of memory or through faulty analysis, find themselves in a new cul-de-sac, let us say in trial No. 10. It will then be partly a chance matter which one will be able to extricate himself in the less disastrous manner. Practically, however, we do not need to assume that the passage is being explored for the first time in order to make allowance for the chance factor. The nature of the situation is such that a subject may be in a passage which he has traversed many times before, and may not recognize the maze formations.

We have concluded in our final statement that chance is not the most determining factor in maze learning. This conclusion is based upon the average results obtained in this experiment. But we are convinced that intelligence does labor under a severe handicap in its attempt to function in the maze. While it is not presumed that the introspective reports furnish anything like an adequate account of the intelligent process, we have found too much variation in the records of the learners using the same method to explain the results on any other grounds. Our chief criticism is, not that chance plays a determining rôle, but that it is a factor which has not been measured, and probably fluctuates. The problem awaits a careful experimental test, in which a

number of subjects will learn a sufficient number of different mazes to permit an estimate of the amount of individual consistency. Assuming that other factors are controlled, any lack of individual consistency may be taken as a rough index of fortuity.

2. Our criticism of the importance to be attached to the data usually recorded and discussed, hinges upon the use made of that data. Mrs. Hicks opens her paper with the following statement: "There are three possible quantitative criteria which can be used in representing the learning process of an animal in the maze, viz., the number of errors, the time of the run, and the total distance traversed."[1] For gross purposes of comparison, these data naturally represent the total showing made by the maze learner. That is, by quoting one or some of these total records made, we may label one learner as 'better' or 'worse' than another. But if the maze experiment is worth while conducting at all, our first concern certainly should be to analyze in more detail the learning process itself; and if this is a legitimate aim, the three kinds of data referred to are insufficient. With the knowledge of what the process really represents, individual or group comparisons would be more in order. It is quite true that our learners were given an ideal of efficiency comparable to that held out to the beginner in stenography, or in any practical accomplishment. Both speed and accuracy were emphasized: he was asked to learn the maze in the 'smallest possible time with the fewest number of mistakes, in the long run.' But both standards are definitely used in measuring the accomplishments of of the stenographer, and they purport to measure nothing but the accuracy and speed with which a habit is acquired. One does not take either his isolated time or error curve alone and proceed to establish that as a criterion of efficiency in stenography, and neither does one take the combined standard as an index of intelligence, unless the term is restricted to 'stenographic intelligence.' If we are to rest content, in a similar manner, with measuring the relative ability of the different types to learn a maze, surely a composite time and error curve,

[1] Hicks, *op. cit.*

such as that suggested by Mrs. Hicks, is required. But we are not sure that such a set of records, even though they were furnished by an inclusive variety of animal and human types, would be of much value to comparative psychology. Their significance, we believe, would be found in their contribution to the problem of comparative intelligence. If we are seeking to evaluate maze records from this point of view, more data is required: we should want to know, for instance, the actual route traversed by the learner. It is conceivable that two subjects may total the same time, error and distance results, and yet one of them show more retracing in the same segments than the other. This might possibly be taken to mean, when the two routes were analyzed, that he made use of greater intelligence.

3. The data afforded by the present experiment by no means made us feel at ease regarding the rôle of chance, nor did it cover completely the point referred to in the preceding paragraph, but it did disclose some of the difficulties we encountered in attempting to analyze and evaluate the respective total time, error and path records. In Table III. the subjects of both groups are assigned places in the columns according to their decreasing order of merit. Some of the interpretations made are as follows: The learner who is thoroughly "good," without qualification or doubt, will be able to show a record indicating relatively few trials, few errors, a small amount of total time, and a small amount of total path traversed. Such a record was made by *B*, *Hv*, and *Hg*. As opposed to this, the poor record will show a large number of trials, much time spent, many errors, and a vast amount of wandering in the maze paths. *McS* and *Lev* have such records. Another point should be noted. The good learner tends to work at a relatively slow rate. That is, he makes fewer errors per minute and covers less path per minute. It does not seem quite so clear, however, that the poor subject necessarily worked at the higher rate, although this is clearly the tendency.

After disposing of the unambiguous good and bad records, how shall we rank the majority of our learners, whose results

place them in fluctuating positions in the scale? One may tend to take a large number of trials, but may be able to show a fair time, error and path record, as did *D*. *Hl*, by way of variation, found 42 trials necessary for his learning, and his time record places him as the sixteenth—out of the 24—in order of excellence. But in the matter of errors and excess distance traversed, he stands in the fourth place from the best. Again, we find this record, by *Hz:* a large number of trials, many errors, much path covered, but relatively little time consumed. With *H*, the net result was a poor error score, a large number of trials; but his time and path records were fair. The table shows that such fluctuations were the rule rather than the exception. The reader, after noting *K*'s record in all columns, would probably rank him as inferior to *R;* but if the time record is to be considered alone, *K* is better by four places.

We should expect, on a priori grounds, such variations, due in part to accident, and in part to differences in character and habits among different individuals. The man of the street distinguishes between the careful, plodding individual and the brilliant though erratic one. Unfortunately, neither he nor the professional psychologist have carried the analysis further. Rather pronounced personal idiosyncrasies, suggesting something of the nature of the ones just mentioned, seem to be reflected in our record sheets. Hence our difficulty in evaluating these records in terms of intelligence. *D* extended the number of his trials willfully—his aim, each time, was to "hurry through and start all over again." *McS* showed no lack of intelligence in her comments. In fact, she is decidedly of the brilliant type. But she lacked patience, and at times made the most rapid movements of any of our subjects, often hurdling out of the run-way altogether. She explained that she does everything that way. *W* is inclined to think things over before acting; *Hy* is noted for being impulsive. Other personal traits seemed to have influenced the different curves.

While we are unable to correlate efficiency in the maze with such temperamental traits or with intelligence in any exact

manner, a few general tendencies were noted. The individual with good general intelligence—our statement is based upon personal estimate rather than upon scholarship—is not necessarily a good maze learner. This seems to hold true especially in the case of the brilliant type: they are mentally alert and competent to profit by past experience in the maze, but they have a tendency to resort too much to a trial and error method. The maze taxes patience and the quick thinker frets about the slow order of things. The individual too much given to analyzing and theorizing is not necessarily proficient in the maze. Too much time is lost in exploring and testing out theories. Intelligence defeats its own purpose. It seems to be just as obvious, however, that stupidity is represented by poor maze records. We experimented with several subjects who are considered to be somewhat backward, and found a general tendency towards inferiority. The mastery of a maze is a learning process calling for intelligence; but human intelligence is complex and aims at different accomplishments, that is, more freedom and variation in the control of the environment. In terms of the maze situation, the learner can deliberately adopt one of several methods of attack, with the consequence that his different values registered do not parallel.

II. Quantitative Results

Our investigation, as originally stated, had for its purpose a comparison of the records of the child and the adult. The numerical data at hand, giving the individual error, time and distance values by trials, invite numerous comparisons. Some of these may be of significance, some may not. Group comparisons, based upon the individual's order of excellence, as shown in Table III., are not clearly suggested; we had not assumed that age differences would be so clearly pronounced as to permit a seried order of excellence, the line of demarcation of efficiency coinciding with the age line. Even in this table, however, the tendency for the children to predominate towards the inferior end of the series is seen. The method of group averages points to more definite group distinctions.

TABLE I

RECORDS OF ADULTS

Name	No. of Trials	Errors		Time		Excess Distance			Av. No. Errors per Minute	Av. Distance per Minute
		Total	Av.	Total	Av.	C. D. S.	True Path	Total		
						4,453.4				
H	41	728	17.7	54′ 12″	1′ 19″	4,453.3	3,483.7	7,892.1	13.4	145.6
W	25	333	13.3	67′ 47″	2′ 42″	3,821.3	4,292.8	8,114.1	4.9	119.7
Mac	33	453	13.7	27′ 10″	49″	4,499.7	3,045.2	7,544.9	16.6	277.6
McG	31	528	17.0	107′	3′ 27″	5,808.3	4,947.2	10,755.5	4.9	100.5
L	41	335	8.2	49′ 53″	1′ 13″	6,015	7,751.7	11,766.7	6.7	235.9
R	48	553	11.5	111′ 30″	2′ 19″	6,419.6	3,157.9	9,577.5	4.9	85.8
Hv	24	217	9.0	48′ 7″	2′	2,663.6	1,415.6	4,079.2	4.5	84.7
Hy	32	711	22.2	108′ 28″	3′ 23″	8,849.1	16,226.4	25,075.5	6.5	231.1
F	46	839	18.2	74′ 19″	1′ 36″	7,738.7	5,693.4	13,432.1	11.2	180.7
Wd	47	473	10.6	63′ 16″	1′ 20″	10,703.3	5,591.6	16,294.9	7.4	257.5
Rh	35	532	15.2	70′ 58″	2′ 1″	5,658.2	5,135.2	9,793.4	7.4	137.9
D	69	538	7.7	64′ 30″	56″	5,835.6	3,024.7	8,860.3	8.3	137.3
Hg	27	213	7.8	42′ 16″	1′ 33″	2,450.8	2,141.6	4,592.4	5	108.6
Hp	27	375	13.9	58′ 51″	2′ 10″	3,707.6	3,354.5	7,062.1	6.3	120.
Total	526	6,828	186	948′ 17″	26′ 48″	78,624.2	66,216.5	144,840.7	108	2,222.9
Average	37.57	487.7	13.2	67′ 44″	1′ 54″	5,616.0	4,729.7	10,345.7	7.7	158.77

TABLE II

RECORDS OF CHILDREN

Name	Age	No. of Trials	Errors		Time		Excess Distance			Av. No. Errors per Minute	Av. Distance per Minute
			Total	Av.	Total	Av.	C. D. S.	True Path	Total		
Hl	10	42	254	6.0	70′ 45″	1′ 41″	2,771.2	1,870.2	4,641.4	3.5	65.6
McS	14	89	1,559	17.5	113′ 35″	1′ 16″	14,992.5	12,883.6	27,876.1	13.7	245.4
Lin	10	38	1,089	28.6	97′ 36″	2′ 34″	9,908.4	7,499.7	17,408.1	11.1	178.3
B	10	20	254	12.7	41′ 50″	2′ 5″	2,290.	1,697.2	3,987.2	6.	95.3
N	13	49	599	12.2	55′ 25″	1′ 8″	8,554.8	3,917.	12,471.8	10.8	225.
T	12	40	520	13.	58′	1′ 27″	5,873.6	6,211.6	12,085.2	8.9	208.3
J	13	50	828	16.5	64′ 4″	1′ 16″	10,742.6	6,317.7	17,060.3	12.9	266.2
Hz	12	40	516	12.9	49′ 3″	1′ 13″	5,201.8	3,795.	8,996.8	10.5	183.4
Lev	11	54	2,171	40.2	173′ 3″	3′ 12″	20,272.7	14,963.1	35,235.8	12.5	203.6
K	11	58	1,331	22.9	90′ 58″	1′ 34″	13,243.	6,614.2	19,857.2	14.6	218.2
Total	116	480	9,121	182.5	814′ 19″	17′ 26″	93,850.6	65,769.9	159,619.9	104.5	1,889.3
Average	11.6	48	912.1	18.25	81′ 26″	1′ 44″	9,385.06	6,576.93	15,961.9	10.45	188.93

Such distinctions are, first of all, open to the criticism that necessarily applies when small groups are used. As a matter of fact, the small number of subjects is limited practically to the younger group; a large number of adult subjects have learned the maze in connection with courses in experimental psychology, and their results lead us to believe that our older group is representative.

All comparisons of the scores of the two groups (Tables I. and II.) indicate superiority on the part of the adults. That such would be the result was obvious to the experimenters, observing behavior, before the numerical records were tabulated.

TABLE III

CHILDREN AND ADULTS—DESCENDING ORDER OF EXCELLENCE

Children are indicated by heavy-faced type

	No. of Trials		Total Errors		Total Time		Total Distance Traversed		Errors per Minute		Distance per Minute	
1	**McS**	89	**Lev**	2,171	**Lev**	173′ 3″	**Lev**	35,235.8	Mac	16.6	Mac	277.6
2	D	69	**McS**	1,559	**McS**	113′ 35″	**McS**	27,876.1	**K**	14.6	**J**	266.2
3	**K**	58	**K**	1,331	R	111′ 30″	Hy	25,075.5	**McS**	13.7	Wd	257.5
4	**Lev**	54	**Lin**	1,089	Hy	108′ 28″	**K**	19,857.2	H	13.4	**McS**	245.4
5	**J**	50	F	839	McG	107′	**Lin**	17,408.1	**J**	12.9	L	235.9
6	**N**	49	**J**	828	**Lin**	97′ 36″	**J**	17,060.3	**Lev**	12.5	Hy	231.1
7	R	48	H	728	**K**	90′ 58″	Wd	16,294.9	F	11.2	**N**	225.
8	Wd	47	Hy	711	F	74′ 19″	F	13,432.1	**Lin**	11.1	**K**	218.2
9	F	46	**N**	599	Rh	70′ 58″	**N**	12,471.8	**N**	10.8	**T**	203.8
10	**Hl**	42	R	553	**Hl**	70′ 45″	**T**	12,058.2	**Hz**	10.5	**Lev**	203.6
11	H-L	41	D	538	W	67′ 47″	L	11,766.7	**T**	8.9	**Hz**	183.4
12			Rh	532	D	64′ 30″	McG	10,755.5	D	8.3	**Lin**	178.3
13	**T-Hz**	40	McG	528	**J**	64′ 4″	Rh	9,793.4	Wd-Rh	7.4	H	145.6
14			**T**	520	Wd	63′ 16″	R	9,577.5	L	6.7	F	180.7
15	**Lin**	38	**Hz**	516	Hp	58′ 51″	**Hz**	8,996.8	Hy	6.5	Rh	137.9
16	Rh	35	Wd	473	**T**	58′	D	8,860.3	Hp	6.3	D	137.3
17	Mac	33	Mac	453	**N**	55′ 25″	W	8,114.1	**B**	6.	Hp	120.
18	Hy	32	Hp	375	H	54′ 12″	H	7,892.1	Hg	5.	W	119.7
19	McG	31	L	335	L	49′ 53″	Mac	7,544.9			Hg	108.6
20	Hg-Hp	27	W	333	**Hz**	43′ 3″	Hp	7,062.1	R-W-McG	4.9	McG	100.5
21			**Hl-B**	254	Hv	48′ 7″	**Hl**	4,641.4			**B**	95.3
22	W	25			Hg	42′ 16″	Hg	4,592.4	Hv	4.5	R	85.8
23	Hv	24	Hv	217	**B**	41′ 50″	Hv	4,079.2	**Hl**	3.5	Hv	84.7
24	**B**	20	Hg	213	Mac	27′ 10″	**B**	3,987.2			**Hl**	65.6

1. *The Number of Trials.*—A correlation exists between excellence—as measured by total gross values—and the number of trials. The children averaged a greater number of trials by a rough ratio of 4 to 3. Eight of the 14 adults learned

the maze in 35 trials or less. While one child mastered the maze in 20 trials, her nearest competitor took 38 trials. The poorest record made by a child exceeded the second poorest by 31; in the case of the adults, the difference was also 31. Granting that the child's record was more of an abnormality than that of the worst adult offender, and subtracting 30 from the sum totaled by the children, their average would, even at that, be 45.

TABLE IV

THE PERCENTAGE DISTRIBUTION OF ERRORS, TIME AND DISTANCE

Trs.	Average Per Cent. of Errors		Average Per Cent. of Time		Average Per Cent. of Distance	
	Adults	Children	Adults	Children	Adults	Children
1	22.59	19.64	21.84	21.82	25.98	20.65
2	13.26	10.04	11.85	7.40	13.62	10.48
3	9.57	16.2	9.12	13.94	10.13	18.
4	7.79	9.99	3.69	7.70	4.73	10.44
5	4.81	7.86	4.49	5.80	6.02	7.60
	58.82	63.73	50.99	56.66	60.48	67.17
6	3.9	8.41	4.04	3.87	4.45	5.55
7	3.27	3.67	2.62	2.75	2.80	4.07
8	3.10	2.40	3.23	4.33	2.52	6.28
9	1.64	1.74	1.94	1.42	1.16	1.74
10	4.27	2.08	3.06	1.87	3.73	2.
	16.18	18.30	14.89	14.24	14.66	19.64
11	3.14	1.88	2.35	1.17	2.80	2.53
12	2.62	2.88	2.12	1.41	2.13	1.93
13	1.34	1.17	1.66	.97	.89	1.07
14	1.90	1.32	1.92	1.22	2.01	1.09
15	2.08	.60	1.94	1.17	1.60	.54
	11.08	7.85	9.99	5.94	8.43	7.16
16	1.70	1.82	1.81	1.59	1.12	1.61
17	1.63	.35	1.99	.72	1.91	.33
18	1.44	.5	1.48	.66	1.05	.23
19	1.26	.26	1.23	.65	1.19	.41
20	1.40	.36	1.62	.68	1.30	.38
Total for first 20 trs.....	92.71	93.17	84.00	81.14	90.14	97.13

2. *Time, Error and Path.*—The error record of the adult again indicates superiority by a considerable margin. The average variation is much greater in the case of the children, due to the contributions of 4 of the 10; but the child with the fifth worst record lacks only 11 errors of reaching the poorest

record made in the adult group. Time and distance show a corresponding balance in favor of the older learner. This balance is the more noticeable with path measurements. Two children exceeded the poorest path record made by any adult. The ratios between the groups for errors, time and distance respectively stand at 1.8, 1.2, and 1.5.

3. *Rate.*—The rate record is suggestive, and partially explanatory of the poorer showing made by the children. Their average time per trial is 1 minute, 44 seconds; for the older learner, 1 minute, 54 seconds—a difference of an amount of time sufficient to permit one familiar with the maze to make the journey from entrance to exit. The child averaged 10.45 errors per minute, while the adult averaged 7.2. A corresponding record is found in the distance results: the child traversed 188.93 centimeters per minute, the adult 158 centimeters. Table III. shows this segregation of the children towards the faster end; and it also shows the tendency towards a correlation between efficiency and a relatively slow rate. The parallel is not exact: *Mac*, with an error record only fair and a path record somewhat better, heads the rate list by virtue of her proportionately lower time record. The case of *J* is somewhat similar. Other variations are represented at both ends of the rate column, but those whose total records were good in all three respects are found to have slow rate scores.

We see nothing in the mathematics of the maze situation to indicate that the efficient learner necessarily makes fewer errors per unit of time, or covers less distance. It is of course possible that of two subjects, one may make twice the number of errors per minute made by the other, and yet equal him in the total errors made; or one of the two may traverse twice as much space as the other per unit of time, both covering the same total amount. *Mac* and *Wd* respectively approximate the first situation; *Mac* and *H* the second. *Mac's* record however seems to be something of an anomaly. The tendency is towards the correlation suggested. Now it is significant that of the six columns given in Table III., only the scores of the last—the amount of space traversed per minute—are under

the direct control of the learner. He cannot will to excel in any measurement but he can move his pencil slowly or rapidly, as he chooses. It seems that his chances of a good total record are better if he chooses to proceed somewhat cautiously. If chance were the sole factor, the likelihood of attaining the exit in any trial would increase as the speed of the subject is increased, and as the ratio between errors and time would be constant, a premium would be placed upon speed. This would presumably be the situation in the case of a purely mechanical device, such as a rolling ball, capable of travelling and turning or retracing at random in the maze paths. Success would be, with such a device, purely chance success. Our situation is that in which the learners are intelligent performers, and while some deliberately assumed

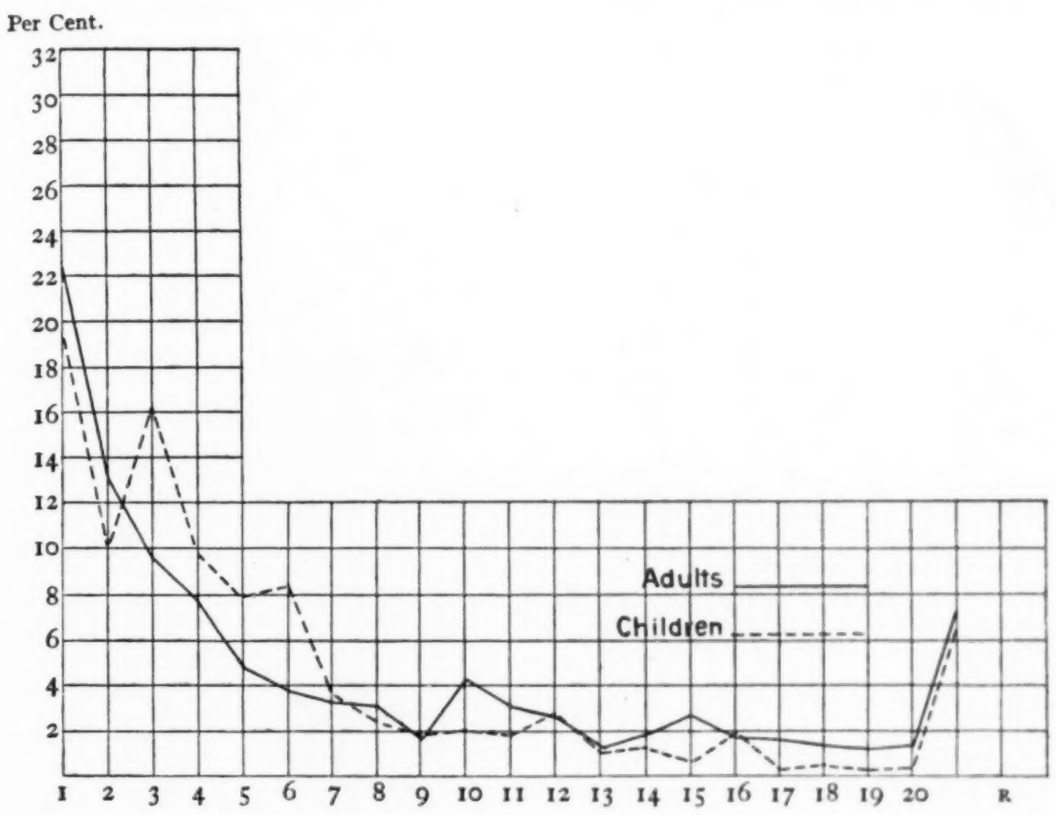

FIG. 2. Percentage Distribution Curve of Errors.

the chance attitude, and relied upon speed, they were as a type unsuccessful. One might consider it advisable to make the first trial a chance matter, and to assume a more rational attitude in the ensuing trials, after the general topography

had been learned, but in practice this plan was not attempted. If intelligence, and not chance alone, functions in the maze learning process, evidently our respective groups either differed in intelligence—maze intelligence—or that function operates more efficiently if given time for the fixing of associations, for memorizing, for analyzing, etc. Our alternatives take the following form: (1) As the two groups differ in age and maturity, they presumably differ in intelligence. The older learners are thinking as they are traversing the maze paths; consequently they tend to go somewhat slowly. The maze does not call into play ideational reactions from the children, and having little to think about they naturally increase their

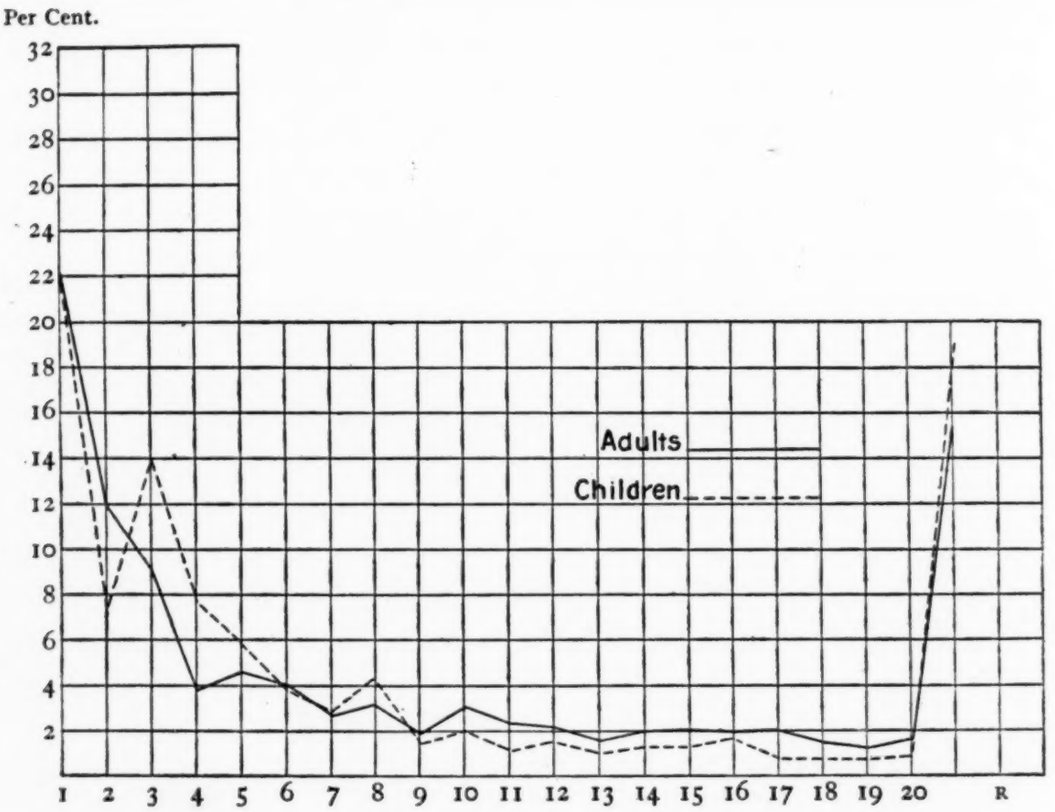

FIG. 3. Percentage Distribution Curve of Time.

speed. If this explanation should hold as stated, it would follow that the child would have little to gain by adopting a slower rate. Their time record would simply be extended. (2) It is conceivable that the child and the adult have practically the same intellectual equipment for the maze. A child of 10 may certainly make a creditable showing in any

isolated test similar to the exercise of any function called for by the maze. But the child lacks the patience, the power of continued application, of the adult. He is more nervous, becomes fatigued the more readily, etc. If this alternative states the psychology of the situation the child would profit by an effort of will leading to more cautious movements in the maze. One may of course push the analysis a bit further and conceive that intelligence operates by choosing the more logical method, and that the child has less ability than the adult to exercise wisely potentialities which they both have in common.

4. *The Distribution of Time, Errors and Distance.*—Table IV. gives the average per cent of error, time and excess distance results for each of the first 20 trials and for the remaining trials considered as a unit. For convenience in ascertaining larger units of distribution, the 20 trials are divided into 4 arbitrary groups of 5 each. Our mathematical treatment of these results is more simple than that made by Carr, as we have assumed that the direct statement of results itself affords a more logical basis for conclusions. One explanation should be given: Carr deals with excess values—scores made in addition to those necessarily registered when the maze is traversed in the shortest possible time without errors. All errors are excess, as is all distance exceeding the length of the true path, and all time over the shortest time made by the individual subject. We preferred to give absolute time results, inasmuch as non-excess time varies with the individual and is probably not accurately represented by his shortest time made. We followed Carr's procedure in subtracting the length of the true path from the path record of each trial, for each individual. While excess distance is by no means confined to cul-de-sacs, all distance exceeding the length of the true path is excess.

The curves given in Figs. 2, 3 and 4, based upon Table IV, show the following characteristics: (1) The adult curve tends to exhibit a more regular and more pronounced slope during the first 10 trials than that of the children. The children's curve is lower in the second trial, but rises above the adults' in the third and shows a more pronounced series of steeples

for the following part of the period, particularly in errors and distance. Values are eliminated early in the series, in both curves, and as the trials are prolonged, the percentage of values yet to be scored tends to be the same for children and adults. All curves from the tenth to the end show general similarity. Those of the children extend considerably further from the line of abscissæ, due to the greater number of trials necessary for their learning. (3) After the period of about the twentieth trial, the percentage of time yet to be disposed of is greater than the percentage of errors and path. The adults have more distance than the children to cover after this trial, but about the same percentage of errors.

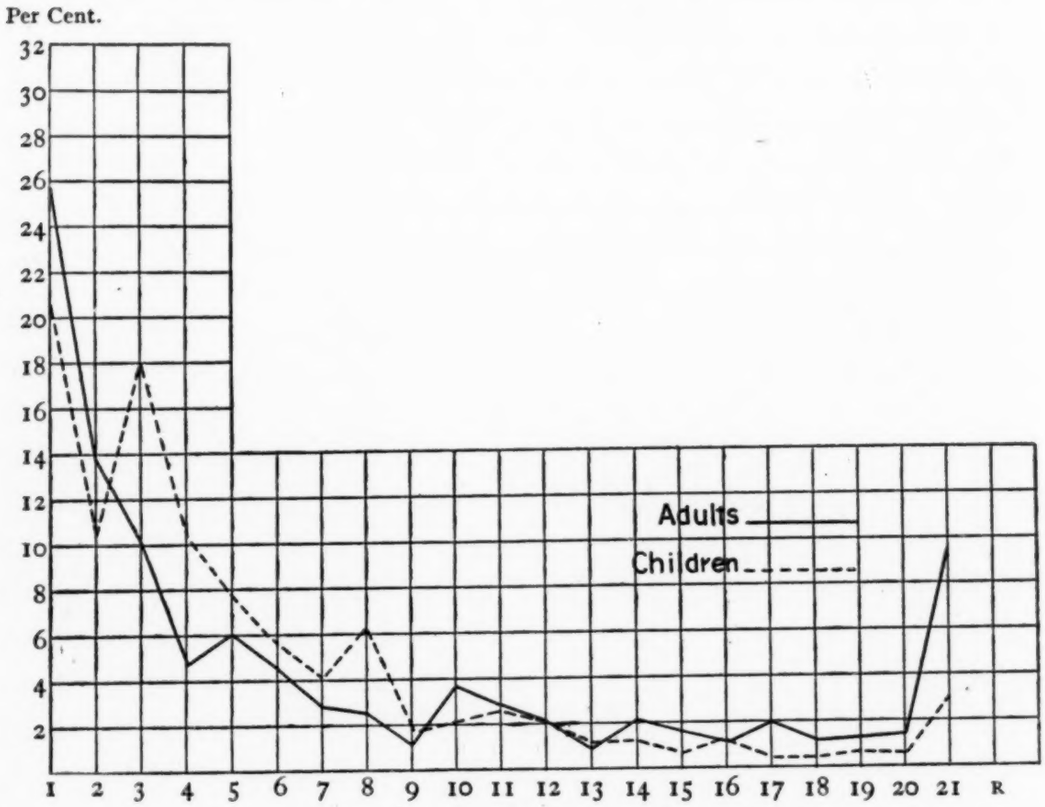

FIG. 4. Percentage Distribution Curve of Excess Distance.

In order to ascertain more clearly the characteristics of good and bad learning as reflected in the distribution of results, we compared the averages of 5 of the best learners, considered as a group, with 5 of the poorest. The first group was made up of *Hv*, *Hg*, *Hp*, *Mac* and *B;* the second, of *Hy*, *McS*, *Lev*, *Lin* and *K*. It will be noticed that 4 adults and

1 child constituted the first group, and that the ratio is reversed in the second.

A detailed statement of the results of this comparison is not necessary, but, stated in terms of the composite curves, it discloses in brief the following: (1) Both the "good" and the "bad" complete the first trial with about the same time and error results—28–9 per cent and 24 per cent respectively. The poorer subjects average more path than the 24 per cent path distance made by the superior group. (2) A somewhat curious phenomenon is found in trial No. 2. The good learners made a relatively poor showing in this trial, in all three values, by a negative margin of about 10 per cent. (3) During the period represented by the first 10 trials, the curves of the better subjects show a more uniform and pronounced slope, while the other curves display more and higher steeples. For these 10 trials however, the curves average about the same height above the base line. (4) From the tenth trial to the end both sets of curves are similar, for the reasons mentioned in our comparison of the curves of adults and children. The good subjects averaged 26.2 trials, the poorer ones 54.2.

It might appear that in comparing the beginning values for the two sets of records the difference in the number of trials should be taken into consideration. The first fifth of the better group would include their first five trials, while the corresponding fifth for the other would extend to trial No. 11. Thus it would seemingly be misleading to compare the second trial of the good with the second of the inferior. But in both sets of curves, the values for the first 10 trials are practically the same; and our notes on introspection and behavior indicate that the respective first 10 trials are comparable. The learning is practically completed by this time, and new factors account for the extension of the poorer set of curves. Hence, we are unable to agree with Carr's argument that the extent of the initial slope is dependent upon the number of trials in the curve.

The individual variations found in the groups again suggest caution, but certain points of consistency justify an inter-

pretation of the results of the comparison. (1) It is obvious that the percentage showing of the first trial is not to be taken as an index of future developments. The trial does disclose the fact that for both sets of subjects more time is eliminated than errors or path. This is merely indicative of the hesitancy with which all learners inaugurate their attempts, before the nature of the experience is known.

(2 and 3) The period included by the first 10 trials represents the critical stage of the learning. Our notes on behavior and our introspective reports, together with the fact that the curves show their greatest points of difference during this period, establish this fact. An analysis of this part of the curve shows two things: (*a*) Bad records tend to occur in individual trials, followed by better trials: that is, the curve takes the form of a series of steeples, rather than that of a high plateau. This holds for all of our records examined, including those of the group under discussion. In the case of the subject with a poor total record, the steeples are higher and more irregular than those found in the good curves. (*b*) Since two of the better group make a creditable showing in trial 2, we may not attach undue importance to the results of this trial, considered by itself. But it is significant that all of the good subjects did relatively poorly in this trial. The net result in practically every case is that the good subjects average a larger per cent of all values in the first two trials than the poorer subjects, for these same trials. All subjects tend to show a slump somewhere in the series, and this tends to come earlier for the better subjects. This, in conjunction with the steeple phenomenon, results in a more uniform and sloping descending curve for the superior subjects in the first 10 trials.

These differences in curve characteristics admit of an explanation in terms of intelligence. Intelligence asserts itself in two forms: (*a*) in the elaboration of schemes of control, or methods of attack—the mature learner is more likely to attempt such expedients as sticking to one side of the path. Such schemes may or may not prove to be well-advised. (*b*) The more intelligent subject does more thinking

in the shape of analyzing maze experiences. He does more discriminating and memorizing, and is more capable in the matter of recognizing maze segments. Moreover, this greater ideational activity begins in the first trial.

The motive in this first trial is the same with all—to attain the exit. Intelligence does not seem to assist in this immediate end, if our conclusion regarding the numerical significance of the first trial holds. But it does function in acquiring a more accurate knowledge of the maze.[1] Two subjects may thus complete trial 1 with the same quantitative results, but with a difference in the amount of retained experience to be applied to the ensuing trials.

It might seem that if this were true the better equipped subject should expedite matters in the second trial, but such does not seem to be the situation. Our explanation takes into account the fact that the intellectually weaker learner is more likely to follow in an unthinking way the route traversed in trial 1, in his second attempt. He does not realize the extent to which he has been retracting from blind endings. The better subject has realized this, and is consequently more uncertain in his second trial. The result is more analyzing in trial 2, more exploring, and a temporary poor showing. He begins his attempt to establish the details of the true path earlier in the series of trials than does the subject who resorts to a less rational method.

It should be remembered that a subject, in any prolonged trial, traces back and forth over the maze segments many times. The first trial, or any similar trial, is thus equivalent to several later in the series. In this initial extended exploration, the learner is not only elaborating his concept of the route, but he is also establishing habits. The less alert subject during his beginning trials tends to build up more false ideas about the maze, and to establish habits for cul-de-sac segments, incorporating them into the true path. In the course of time these are forced upon his attention. The result is the necessity for the reconstruction of ideas and for the breaking up of inadequate habits.

[1] See page 26.

The weaker subject thus not only finds analysis more difficult, but he is handicapped by being forced to "start all over again," to use a quotation which more definitely expresses his difficulty. This ordeal is passed through in trials 3–10. That the quantitative expressions of this period take the form of irregularities rather than that of a uniform high score of errors, time and distance is another matter. Explanations of steeple phenomenon were attempted in the writer's monograph article. As far as the child is concerned, a prolonged trial during this period is usually explained by him on the grounds that he was "lost." This means less ability to recognize. Further analysis discloses the fact that the learning takes place in terms of the acquisition of the ability to recognize and anticipate cul-de-sac openings, rather than the cul-de-sacs themselves. Either a good or a bad learner, at any stage of the process, may become lost; but the better learner is more quick to reestablish his orientation, after he regains the true path.

Several modifications should be appended to our explanation. We wish particularly to refer to the chance element, criticized in our preliminary statement. That the better records of the adult, generally speaking, are to be accounted for in terms of intelligence is a conclusion supported by our various data, but the handicap of the rational element is emphasized in the results of the first trial, and in the lack of consistency found in many of the trials that can not be explained on other grounds. Superior reasoning does not facilitate the attainment of the exit in the first trial, as it should do if it were an unhampered functioning agent. Practically all of the subjects expressed surprise when the exit was reached for the first time, thus indicating the haphazard nature of this trial. Then we must make due allowance for a chance discovery of the true path early in the first trial. The good reasoner would of course follow this in the second and succeeding trials. *McS* and *Hv*—about our poorest and best subjects respectively—both made exceptionally good initial records. *Hv* was the more successful in this trial, but her total success is out of all proportions to the

accomplishments of *McS.* Hence we assume that *Hv* made better use of the knowledge acquired in this first trial, as well as in the ensuing ones. Her first drawing (section III.) was infinitely superior to that of *McS.* *Hv* was, incidentally, our most mature and experienced subject. We conclude that both were favored by chance at the start, but that the intelligent factor operated in *Hv's* favor as the trials progressed. But we find that many of our subjects who to all appearances reasoned well in the maze, did poorly at the start so that they were evidently working under a greater chance handicap than *Hv*, unless we wish to credit her with more than exceptional intellectual ability in the maze. This we are unwilling to do.[1]

Differences in fatigue, temperament, general emotional tone, entered into this period to some extent, although we show reasons later for thinking that they were more influential towards the close of the series.

Again we should modify the implications of our explanation with the statement that a bright child may show evidence of as successful an intellectual performance as that evinced by the normal adult. In a mental test employing the maze subject *B* certainly would qualify as an adult.

More uniformity is found in the trials from the tenth on, as result (4) stated. The important turns of the true path had been learned by this trial, and the rest of the effort was centered upon minor difficulties. By the stage of about the twentieth trial, all subjects were definitely motivated by a

[1] The writer, in his monograph study of the maze process, contributed one of the many criticisms directed against Thorndike's principle that a pronounced slope of the learning curve indicates intelligence. We held that the slope of the maze curve is determined by the pattern of the maze. Carr objected to Thorndike on the grounds that the number of trials conditions the apparent slope of the maze curve. That criticism has already been stated and discussed. Our curves for the maze described in this paper show general points of similarity, as contrasted to curves from other mazes, hence our first point seems to be substantiated. In a corresponding manner, the curves for any given problem are characteristic (compare Ruger's puzzle curves). But within the curves of a given maze Thorndike's principle seems to apply. Assuming that the reasoner could master the maze—or the puzzle, for that matter—in the first trial, his curve would drop to the base line. Purely accidental discovery in the first trial would not produce this curve unless retention were perfect in ensuing trials, but that would presuppose the rational factor in a high degree of efficiency.

special desire—to run the 5 consecutive trials without error. An error in the fifth trial, following 4 perfect trials, meant a series of at least 5 more. In its intelligent aspects, this was a task of practically equal difficulty for both children and adults. But the greater fatigue shown by the children, the more obvious signs of nervousness on their part, began to tell and contributed largely to their added trials.

5. *Error and Distance Comparison.*—The ratio between the number of errors and the distance traversed was a matter of interest in the light of some conclusions made by Carr. In his error and distance comparison, it was found that his groups, human and animal, eliminated distance more than errors, but that the period during which the elimination took place varied with the groups. For the adult humans, it was in the first half of the learning; for the animals, in the second half. The children occupied a mean. This was interpreted as a quantitative representation of the intelligent factor: the humans, more particularly the adults, learned in time to recognize cul-de-sac entrances, after turning into them, without the necessity of traversing the entire cul-de-sac distance. By the method of scoring, this behavior would enroll as many errors but less distance. Neither our ratios nor our notes on behavior indicate that such a numerical index of intelligence is to be found in our results. In the curve of the adults, the path record gains on the error record 2 per cent in the first quarter, loses this amount in the second, and is behind about the same amount at the end of 20 trials. For the children, more distance than errors is eliminated in the first quarter by 4 per cent, in the second quarter by 1 per cent., and at the end of the last trial, by 4 per cent. The greater elimination of distance is only obvious in the case of the children, and not to the extent to be regarded as significant.

Particularly noticeable was the behavior tendency of all individuals invariably to go to the end of every cul-de-sac entered. This may be explained either upon the grounds that we employed a pencil maze, not one through which the subjects walked, or that our maze was more difficult by way of homogeneity and number of passages than Carr's. The learn-

ing process in the pencil maze goes on essentially in terms of the anticipation of turns to be avoided, as the subject is going through the maze, rather than in terms of the recognition of cul-de-sacs once entered. Such reports as the following were consistently given to us: "I learned not to turn down at that point, but to keep straight ahead." With the anticipatory idea in mind a few centimeters ahead of the turn in question, the learner was able to avoid it. The subject as a rule did not recognize that he was off the true path until he had reached the end of the cul-de-sac; or if the premonition of danger was given to him, he usually decided to "go ahead anyhow" for purposes of verification. By comparing the diagrams of the two mazes, one may understand the difference in behavior found by Carr. The point at least would make one hesitate before assuming the maze learning process is the same with any type of maze.

6. *Time.*—The curves demonstrate the noticeable extent to which time is eliminated more slowly than errors and distance, for both groups. From the twentieth trial to the end the child and the adult had respectively 18 and 16 per cent of their time yet to spend in making less than 10 per cent of the errors. This indicates a common element in the learning process rather than a group distinction. The maze was practically mastered by all by the twenty-fifth trial. The concern of the learners from this period on, was to make the 5 trials in succession without errors. A mistake in the fifth trial without errors meant 5 more trials. Hence extreme caution—*i. e.*, a slow rate—was universally in evidence in the latter trials.

III. Introspective Contributions

Our attempt to obtain and record an introspective account of the learning process necessarily involved a rehearsal on the part of the subject of his intelligent processes after each trial, and the attendant danger of learning between trials. Since this danger could not be avoided, we endeavored to make it a common factor with all the learners by making the introspections as systematic and uniform as possible. As a matter

of fact, the end of the trial generally found the subject interested in his performance and more than willing to discuss it. The danger could not have been avoided merely by dispensing with this data.

The aspect of the subjective learning process most easily obtained is the learner's progressive knowledge of maze paths and cul-de-sacs. This was given to us by the subject primarily through a series of drawings or diagrams, supplemented by oral explanations. Drawings were asked for after the first and last trials, and after intermediate trials in which acquisition of new points was reported. At least 5 drawings were made by each subject. We had given consideration to the possibility that a mere difference in the ability to draw might unconsciously affect any conclusions suggested after comparing the two sets of drawings, but since the diagrams were explained and defects obviously due to lack of ability with the pencil corrected, this difficulty was obviated.

While the drawings were sufficiently clear, the matter of passing adequate and fair judgment upon them did prove to be rather difficult. Some included too many path-ways, some not enough, and the proportions varied independently of the accuracy with which the sequence of paths was indicated. We attempted a double scoring method, that omitted the evaluation of proportions and a consideration of cul-de-sac paths. These cul-de-sac passages were hazily drawn by all of the subjects, and that fact in itself indicates that the thinking was essentially concerned with the true path and the location of the "bad places," or false turns—a point that receives ample substantiation in their verbal statements. The subjects were scored, (1) upon the number of segments correctly included, (2) upon imaginary segments believed to belong to the route.

The results of the first and final drawings are recorded. The intermediate diagrams for each subject seemed to be of a degree of excellence represented by a mean between his initial and final attempts. Our method gave us a comparison between the two groups that certainly would be in harmony with the verdict of anyone who should examine all of the

drawings. The adult disclosed a superior knowledge of the maze, and also showed a greater tendency to incorporate paths not actually belonging to the maze. Out of the 22 segments of the path, the adults averaged at the end of Trial 1, 8.9 paths drawn correctly, 10.3 excess; the children were able to draw 6.1 paths, and included only 3.2 unnecessary runways. These proportional amounts of actual and excess paths gradually approached the final drawings, which show less dissimilarity. At the final trial (that is, for each subject) the adult included 17.5 out of the 22 paths, and retained 4.3 imaginary paths; the child's results were 18.2 and 2.9. *Hp*, an adult, was scored 7 on his final drawing. As the adult's drawing judged to be the next poorest received a grade of 17, and as the child's final drawing ranking lowest was graded as 16, we would, by omitting this one case, judge the two sets of final diagrams to be about of the same merit, or rather, rank those of the adult as slightly superior. We were unable to decide in favor of either group in the matter of proportions. The adults showed a greater tendency to recognize all the turns of the maze as being right-angled turns, to estimate the size of the maze and the lengths of its various path-ways more correctly.

A tendency towards correlation between accurate drawings and good maze records is in evidence, but it is by no means unambiguous. *Hv* made by far the best drawings, *Hp* made the poorest, of any of the subjects. *McS* made miserable diagrams, and those of *J*, *K*, *Hy* and *Rh* were only slightly better. *B*, an excellent subject, was only mediocre in this respect; *Hg* was somewhat better. We had demonstrated before that only an uncertain positive relationship obtains between ability to traverse a maze route and ability to describe it, either verbally or graphically. One may complete the learning and be unable to describe more than the general course of the route.[1]

With the greater knowledge of the maze displayed by the adult there was also sufficient evidence of more ideation—more thinking. This does not necessarily mean that the adult

[1] Perrin, *op. cit.*, p. 28.

paid closer attention to his task. The distinction is rather to be found in the more pronounced tendencies towards theories and interpretations—probably represented by the greater number of superfluous pathways noted in the drawings. Reports of schemes and methods came predominately from the adults—plans to "wiggle" the pencil, to follow one side of the path at a time, to explore systematically, to count sections of the path. More ideas and suggestions, whether legitimate or not, seemed to occur to the adult than to the child during the trial.

Although the child and the adult represent different stages of intellectual equipment, these degrees are separated by a fluctuating line of demarcation. Subject *B* has already been mentioned as a child who gave all indications of mature thinking. The maze experience is unique in its presupposition of any kind of technical knowledge. A few elementary spatial concepts, such as those of a "T" turn or an "L" turn, and a primitive knowledge of spatial relationships on a plane, constitute all of the capital stock of information necessary. And these concepts may be acquired for practical purposes in the first trial. We see no reason for assuming that differences in mere knowledge background should count in favor of the adult. If the knowledge factor is reasonably well equated, our distinction remains to be expressed in terms of associational and retentive activities, of a general rather than a specific sort, inasmuch as the pencil maze presented a novel experience to all of the learners. Distinctions in these abstractions, found between the human ages of 10 and 21, are to be made only with considerable reservations.

IV. Methods of Attack

Two methods of attack were fairly well defined among our learners. They represent the extreme types of such behavior as is under the control of the learner, and while intermediate stages were described, the majority of the subjects may be practically classified as falling into one of the two groups. The terms "rational" and "trial-and-error" have recently come in for some long-needed criticism,

and we feel that they are inadequate and misleading for our classification, but our two methods approximate something like the antithesis connoted by these phrases. A subject may avowedly and deliberately rely upon chance in his effort to discover the correct sequence of maze paths. He will hurry through the path-ways, turning at random where opportunity affords. He will make no special effort towards analyzing. As the result of his random endeavor, success will eventually come, and the correct turn will tend to "stick" in his mind. Even then, he may not exert himself in any attempt to remember the turn, but will trust to a repetition of the experience to guide him the next time. As opposed to this, we find the studious, analytical type. We certainly should not say that the first type exhibits a non-rational behavior; neither is it implied that the learner of the second type does not consciously fall back upon the element of chance. All testify to the "trial and error" nature of the process. The distinction is made more difficult by the appearance of the subject who chooses to work rapidly in the maze, gambling upon the greater likelihood of welcome discoveries through rapid excess activity, but attempting as much analyzing as his behavior will permit.

No consistent correlation holds between method and the age of the learner. It should be stated that in our preliminary directions we urged the utmost freedom in the matter of method, suggesting only that the learners employ the tactics which they think will produce the best ultimate results, and notifying them of the three records to be kept. The greater proportion of both groups were decidedly deliberate. The children certainly were not open to criticism on the charge of slip-shod method. *B*, *N*, *T*, *Hl* and *Hz* were thoroughly studious and conscientious. *Lev*, *Lin* and *K* were difficult to classify. *J* attempted the combination of rapid movements with seemingly intense mental application. *McS* consistently sacrificed everything to speed, and represents the extreme of the trial and error type. The adults do not all fit themselves readily into our scheme of classification. *W*, *Hv*, *R*, *L*, *McG* and *Wd* all studied their problem. *H* and *Hg*

did so most of the time, but were subject to aberrations towards the end of the series. *Mac* worked rapidly but carefully. *Hy* was somewhat erratic, generally tending to emphasize random try-outs. No case similar to that of *McS* was found among our adults, although an identical instance was furnished by R. B. O., reported in our previous paper.

That a rational method is necessary for efficient maze learning is obvious. Such an intellectual attempt may easily defeat its own purpose if it runs to over-speculation. It then leads to excessive deliberate exploration and to highly elaborated concepts of maze formations that do not exist. The tendency, by no means confined to the laity, to disparage the performance of the adult in mental tests, finds some support, but more adverse criticism in our conclusions. If theorizing is disastrous in the maze, practical thinking counts largely towards success, and other things being reasonably equated, we should decidedly credit the adult with the more average ability of this sort.

V. Fatigue, Motor Stability and Emotion

Our statement has so far explained the superior excellence of the adult from the point of view of more efficient intelligence. A group of non-intellectual factors however contributed to the juvenile inferiority. Possibly they influenced total results as much as rational activities. These factors were more obvious than tangible, but we have conveniently referred to them under three arbitrary headings: (1) Although the children displayed an amount of effort towards the solution of an exacting task that almost amounted to heroism, their patience began to be considerably strained by the fifteenth or twentieth trial, and signs of greater fatigue were unmistakable. As the number of their trials was greater, this may be taken as either cause or effect. But by trial 20 they had the route practically under as good control as the adults, and they therefore took more trials in eliminating minor errors, the nature of which they perfectly well understood. Hence we conclude that lack of patience, due mostly to fatigue, prolonged the trials. (2) The test was after all of the nature of a class-room

exercise, and our adults were selected from a somewhat restricted group—with one exception they were university students, hardened to the routine of laboratory habits. It was unfortunate but unavoidable that the trials were made in sequence. These facts would suggest the possibility of the superior organization of general habits on the part of the adult. (3) Emotional tone seemed to vary with the success of the learner, irrespective of his age. Interest fell when success was slow in coming, and something almost amounting to elation marked the discovery of correct reactions. The adult seemed to suffer from the effects of discouraging results as much as the child; both assumed with equal emphasis that their mentality was being examined, and were correspondingly sensitive in the matter of their scores. Although indications pointed to more similarity than difference between the groups in this respect, we are not prepared to assert positively that an intrinsic distinction is not to be found.

VI. Summary of Results and Conclusions

The maze experiment affords opportunity for a comparison of the learning curves of different human and animal types, but it is open to serious criticism in that it places a premium upon purely chance discovery—a factor which, whatever its importance may be, has never been measured.

A standardization of the technique of conducting the maze experiment, and a more detailed study of the significance of its learning curve, are needed before its value in comparative psychology is established. It is suggested that a study of the actual route traversed by the learner may yield valuable results, and that a combination time and error curve should be employed in evaluating quantitative records.

On the basis of total error, time and distance results, and the number of trials, the adult of 21 is superior to the child of 11 in the pencil maze. Only loose correlations are found in these criteria, in the records of our learners, hence our standards of excellence are not definitely established.

The factors counting for the superiority of the adult are more effective intelligence and more motor stability.

A graphic representation of intelligence is found in the initial part of the learning curve; the effect of fatigue and motor control are found mainly in the last part of the curve. The intelligent learner tends, (1) to make a relatively poor record in the first two trials, (2) to eliminate values gradually, the result being a curve which shows a relatively more pronounced initial slope and absence of steeples, when compared with the curve of the child. Fatigue shows its effects more towards the end of the learning by extending the number of the trials, after the maze has been actually learned. This prolongs the curve, especially that of the child. The crucial part of the learning for both the child and the adult is represented by the first 10–15 trials; the ensuing trials represent the effort of the learner to perfect the route.

The quantitative differences in the two sets of records are substantiated by the introspective and observational data, which indicate the adult's superior knowledge of the maze, his more efficient regulation of the rate of movement, and evidence of less repetition of maze path-ways.

All distinctions found to hold between the child and the adult are extremely relative. Some of the children seem to compare in every respect with the normal, average adult; and the reverse is found to be true, especially when the records of adults obtained in a previous experiment are examined.

THE EFFECT OF UNIFORM AND NON-UNIFORM ILLUMINATION UPON ATTENTION AND REACTION-TIMES, WITH ESPECIAL REFERENCE TO STREET ILLUMINATION

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A. Introduction

The problems of street illumination have not been approached hitherto from a psychological standpoint. The principal considerations have been photometric and æsthetic, but it is evident that these are insufficient. Mere ability to discern surface irregularities, vehicles, pedestrians or obstacles on the street or sidewalk does not insure safety unless the reaction to these stimuli is normal. If, under a given system of illumination, the reactions of the driver or pedestrian are retarded and the degree of attention diminished, the danger is manifest. From casual observation it would appear that some such factor is present. Many automobilists speak of feeling "sleepier" under certain varieties of street-lighting. Moreover on theoretical grounds the monotony of some lighting systems might be expected to influence the mental state.

During the summer of 1914 the writer was connected with an experimental study of street illumination conducted by the Street Lighting Committee of the National Electric Light Association on Intervale Avenue, New York City. It was found feasible to test the attention and reaction-times of a subject walking on the street under various conditions of illumination. The results thus obtained proved rather significant and the following winter the problem was reduced to laboratory form and experiments conducted in the Harvard Psychological Laboratory. The following article thus comprises two parts, an account of the experiments performed

upon the street and of the supplementary experiments in the laboratory.[1]

B. Experiments on the Street

I. *Equipment*

The following experiments were conducted at Intervale Avenue, Bronx, N. Y. Two sections of the avenue, each a third of a mile in length were equipped for experimental purposes. In one section series circuits fed lamps mounted at the curb 14 ft. above the street, staggered at 50 ft. intervals, with an alternative system of centrally hung lamps 24 ft. above the street at 150 ft. intervals. In the other section the lamps were suspended from span wires and could be moved to any position transverse the street, or from a height of 18 to 30 ft. Provisions were made for mounting series lamps at 75 ft. intervals with two series circuits feeding alternate lamps. The circuits were operated from tub transformers in a temporary sub-station supplied by a 2,200 volt circuit from the Hunt's Point sub-station of the New York Edison Company. The street was 60 ft. wide from curb to curb and paved with Belgium block. Dwelling houses, small apartments and a few stores lined the sides. The street was reasonably free of traffic during the evening.

II. *Method.*

Certain members of the Street Lighting Committee conducted experiments designed to bring out the ability to discern surface irregularities and obstacles in the street under various conditions of illumination. The writer confined himself to attention, reaction times and motor coördination. The efficiency of the eye, while undoubtedly of importance where a person is exposed to street illumination for a prolonged period, as in the case of the motorman, was not studied. It was deemed advisable to restrict the experiments to relatively short periods of continuous exposure to a given illuminating system—the condition encountered in the case of the average pedestrian.

[1] The writer expresses his obligations to Professors Hugo Münsterberg and Herbert S. Langfeld and to Messrs. Preston S. Miller and W. F. Little, of the Electrical Testing Laboratories, New York City.

To measure the quickness of response to unexpected stimuli and the ability to make a quick association and carry out the appropriate movement, the auditory choice reaction was adopted. Auditory rather than visual stimuli were used because they left the subject free to look down the street as he would normally. It was desired to reproduce as closely as possible the situation that is met in practical conditions, and with red and green lamps for stimuli the subject would tend to keep his eyes fixed on the point from which the stimulus was to come rather than directed down the street. Two electric bells of distinctly different pitch were connected in parallel. In series with them were two presselles at the end of 10 ft. of lamp cord, a signal magnet and dry cells. The magnet and a 100vd. electric tuning fork recorded on a kymograph. The whole apparatus was mounted rigidly and clamped to a table built on the truck of a baby carriage. The subject walked down the traffic line midway between the center of the street and the curb, holding a presselle in each hand. The experimenter followed at a distance of about 8 ft. wheeling the apparatus. The subject was instructed to look down the street as he would under ordinary conditions, and when he heard the "cow-bell" to release the right hand and when he heard the "door-bell" to release the left. Trials for the right and left hands were given in irregular orders and at irregular intervals without warning. Right and left reactions registered similarly and were averaged together, approximately the same number of both being given in a series. Incorrect reactions were few (not more than 5 per cent.). The subject reported mistakes and such reactions were omitted from the results. The subject walked continually except when it was necessary to stop a moment to shift the drum of the kymograph. The subject always passed through the complete distance between lamps or multiples thereof, depending on the spacing, so that reactions were made at approximately all degrees of illumination under a given system. Fifteen to twenty reactions constituted a series according to the capacity of the drum, *i. e.*, from 60 to 80 reactions for a subject on a given comparison.

A form of short exposure test was adopted as a measure of attention. A box was constructed 30 × 30 × 30 cm. with a 15 cm. tapering extension on the top through which the subject could look at a field 45 cm. from the eyes. The interior was illuminated by two battery lamps screened from the eyes of the subject and controlled by a switch and an opening at the bottom of one side of the box allowed cardboard material to be inserted. This tachistoscope was clamped on the baby-carriage. Simple geometrical figures such as a square, circle, trapezoid, etc., constituted the material. It was desired to test not merely memory for a series of separate figures, but rather the ability to analyze them out of a complex group, which necessitated a high degree of attention. Accordingly five were drawn in outline with ink on light gray cardboard 30 × 30 cm. so that four of them touched at the center of the card, above, below, right and left, and the fifth was concentric with the field. The subject placed his eyes at the opening and one second after the signal "ready" the field was exposed for one second by closing and opening the switch in time with the swings of a half-second pendulum attached to the side of the tachistoscope. Immediately after the exposure the subject drew upon a pad as many as possible of the figures in the correct position.

Exposures were made at intervals of 15 ft. along the traffic line, a new permutation of objects being shown each trial. When walking between trials the subject kept his eyes directed down the street as he would normally. In scoring the results a figure in the correct position was counted 2, and a figure merely disoriented or displaced less than a quadrant from the correct position counted 1. The benumbing or quickening effect of a given illuminating system on the mental setting would presumably not disappear in the few seconds that elapsed while the subject was lowering his head to the opening of the tachistoscope and awaiting the exposure after the signal "ready." The gray cardboard was used as it afforded less contrast with the street surface upon which the subject had been gazing a moment before. Ten exposures usually constituted a series, in some cases twenty—*i. e.*, forty or eighty

trials on a given comparison. In some instances several subjects (never more than five) were used at the same time, each one stepping aside to make his drawing while the next one observed the figures. Care was taken that no exposure should be made while there was any distracting motion among the subjects.

As a measure of motor coördination a "three-hole" test was employed. A board 20 × 20 × 1 cm. had three 12 mm. holes bored in it at the vertices of an equilateral triangle 10 cm. on a side. A brass plate covered the bottom of the board and was connected in series with a signal magnet and dry cells. A tapping stylus formed the other pole of the circuit and contacts made at the bottom of the holes were registered by the signal magnet on the kymograph. The subject was instructed to insert the stylus in each hole successively as rapidly as possible, being sure to touch the brass plate at the bottom of each hole before passing to the next. The results were thus reduced to terms of speed. The test was always performed under a lamp where the holes could be plainly seen, the inside of them being painted black to render them more visible against the white board. The actual illumination of the board was not of importance as long as the holes were clearly visible and there was no distracting reflection of light from the board. The subject tapped for 30 seconds from the signal "ready—go" until the signal "stop." Four or five trials were given after each choice reaction series. Between trials the subject looked down the street.

The main interest of the experiment was in the comparative results under any two systems of lighting. With each subject two equipments were used on the same evening, so that variables such as temperature, weather and physiological condition were fairly constant. The tests were always performed in both time orders—equipment *A* followed by equipment *B* and then after an interval of 10 or 15 minutes, *B* followed by *A*. The average of the two series on the first equipment was compared with that of the two series on the second. A short practice series was given each subject before proceeding

to the street for the first series on a given method. The subject always walked for 5 minutes on a given street before being tested.

For all the above experiments only short sections of the two streets were used. This was because one street passed through a square with cross traffic and a number of brightly lighted stores. Two short sections were selected which were fairly similar in conditions along the side. In one case there was a small garage and in the other a delicatessen store on the side of the street used and these were frequently closed during the later hours. Precautions were taken against undue distraction in all the experiments. No trials were made when a vehicle of any sort was passing or when there was any unusual disturbance on the sidewalk. Conversation during a series was avoided. In only two or three cases were the subjects at all aware of the trend of previous results. All possibility of suggestion was avoided. The subjects were employes of the Electrical Testing Laboratories of New York City, and of the New York Edison Company, the majority of them between twenty and thirty years of age. The experiments were performed between 8 P.M. and midnight.

III. *General Results*

Experiments carried on by the above methods indicate that of the illuminating factors influencing the attention and mental reactions, uniformity and non-uniformity of illumination are paramount. The main thesis that seems established by the experiments on the street is that under a distinctly non-uniform illumination (with a mean variation in foot-candles of upwards of 50 per cent.), the attention of an individual walking through the street during a period of 20 to 30 minutes is at a higher level and his reaction time is quicker than when he is walking under a distinctly uniform illumination during a similar period.

Table I gives the results of all the experiments in which a markedly uniform system of lighting was compared with a markedly non-uniform. In the first column are given the two equipments compared on a given evening. Nitrogen gas

filled Mazda lamps were always used. To produce uniform illumination, 80 candle power lamps in Holophane units with refractors were mounted 14 ft. above the curb, staggered at 50 ft. spacing, or 250 c.p. lamps in Holophane units with refractors were mounted 24 ft. above the center of the street at 150 ft. spacing. The non-uniform illumination was usually produced by 250 c.p. lamps in diffusing globes (the Edison AB unit) centrally hung 24 ft. above the street at 150 ft. spacing, or by 400 c.p. lamps in Edison units mounted 30 ft. above the street 7½ ft. out from the curb and staggered at 300 ft. intervals. In a few cases bare lamps were used. In the first column of the table the first figure indicates the mounting and the second the spacing followed by the type of unit and the candle power. The next two columns indicate roughly the difference in uniformity of the two equipments under comparison. Measurements of horizontal and vertical illumination were made with portable photometers at thirty stations on a typical block of the street between two lamps for each of the equipments used. The mean variation of these thirty measurements expressed as a per cent. of the mean illumination represents roughly the degree of uniformity of a given system. In the table the per cent. mean variation of the non-uniform street is divided by that of the uniform that was compared with it for both horizontal and vertical illumination. The larger figure in the table thus indicates a greater difference in uniformity between the two equipments compared. The remaining colums give the results of the individual series on each of the three methods. The names of the subjects are not given because, with a few exceptions, the same subject did not participate on more than one evening and consequently stress is to be laid not on individual differences but on general tendencies. The three columns under each group give respectively the per cent. of superiority of the average results of a subject under the non-uniform equipment, the difference between his averages on the two equipments divided by the probable error of difference, and the mean variation (in per cent.) of the results under the uniform lighting divided by that under the non-uniform. The mean variation

TABLE I

Equipment				Difference in Uniformity		Choice Reaction			Tachistoscope			Motor Coördination		
Mount	Space	Type of Unit	C.P.	Horizontal	Vertical	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform
24	150	Edison	250	2.32	1.25	30.0	5.5	1.07	13.6	2.2	1.52	12.1	4.6	1.49
14	50	Holophane	80	...	...	28.4	7.4	.88	− .7	.2	1.86	5.3	1.6	3.46
..	..	.....	..	...	...	9.2	2.8	.86	5.1	.7	1.18	20.0	7.7	2.02
..	..	.....	..	...	...	6.4	3.2	.75	28.0	3.4	2.20	5.4	1.7	1.62
..	..	.....	..	...	...	9.7	2.6	.76	5.7	1.0	1.51	5.5	1.1	.40
..	..	.....	..	...	...	....	...	...	....	...	...	7.1	1.4	1.07
..	..	.....	..	...	...	....	...	...	....	...	...	− .7	.1	1.10
..	..	.....	..	...	...	....	...	...	....	...	...	− 8.1	1.0	.56
..	..	.....	..	...	...	....	...	...	....	...	...	10.1	1.7	1.09
..	..	.....	..	...	...	....	...	...	....	...	...	− 5.6	1.1	1.57
30	300	Edison	400	3.06	2.09	46.2	6.4	1.23	13.1	1.9	1.13	2.0	1.3	1.14
14	50	Holophane	80	...	...	11.7	3.5	1.79	9.6	1.8	.74	− .4	.3	1.98
..	..	.....	..	...	...	16.8	2.5	.69	3.9	.6	1.06	4.4	1.5	2.08
..	..	.....	..	...	...	10.7	3.1	1.25	− 5.8	.8	1.15	....	...	...
..	..	.....	..	...	...	....	...	...	14.1	3.9	1.01	....	...	...
..	..	.....	..	...	...	....	...	...	− 3.8	.5	.82	....	...	...
..	..	.....	..	...	...	....	...	...	5.7	.9	1.08	....	...	...
..	..	.....	..	...	...	....	...	...	1.0	.2	2.02	....	...	...
..	..	.....	..	...	...	....	...	...	11.0	1.5	.94	....	...	...
..	..	.....	..	...	...	....	...	...	16.8	9.9	2.00	....	...	...
..	..	.....	..	...	...	....	...	...	10.3	2.9	1.70	....	...	...
..	..	.....	..	...	...	....	...	...	2.0	.2	1.90	....	...	...
..	..	.....	..	...	...	....	...	...	14.9	1.9	1.20	....	...	...
..	..	.....	..	...	...	....	...	...	16.8	2.0	2.04	....	...	...
..	..	.....	..	...	...	....	...	...	− 2.7	.5	1.27	....	...	...

Equipment				Difference in Uniformity		Choice Reaction			Tachistoscope			Motor Coördination		
Mount	Space	Type of Unit	C.P.	Horizontal	Vertical	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform
24	150	Edison	250	1.32	1.25	15.1	2.8	.76	— 8.6	1.5	1.26	5.5	1.6	1.42
24	150	Holophane	250	...	...	8.5	2.3	1.05	7.2	1.1	.78	3.4	2.4	1.03
..	..	.....	..	...	...	15.4	3.9	1.03	1.8	.3	1.11	5.0	3.1	1.05
..	..	.....	..	...	...	13.2	3.5	1.01	13.3	2.3	1.54	1.0	.9	1.00
..	..	.....	..	...	...	15.1	5.3	1.25	9.2	1.7	.90	6.3	1.1	.68
..	..	.....	..	...	...	14.1	3.4	1.27	20.2	3.2	1.22	1.3	.6	2.16
..	..	.....	..	...	...	....	...	...	17.5	2.1	1.73	....	...	...
..	..	.....	..	...	...	....	...	...	6.8	1.1	1.34	....	...	...
30	300	Edison	400	1.74	2.10	....	...	...	....	...	...	4.0	1.9	.89
24	150	Holophane	250	...	...	....	...	...	....	...	...	1.2	.4	2.63
..	..	.....	..	...	...	....	...	...	....	...	...	— 5.3	2.2	2.08
24	150	Edison	400	1.96	1.34	32.3	6.5	.71	5.1	7.7	.73	....	...	...
14	50	Holophane	80	...	...	....	...	...	....	...	...	....	...	...
24	150	Edison	250	1.23	.90	10.8	2.3	.64	....	...	...	....	...	...
14	50	Bare	80	...	...	....	...	...	....	...	...	....	...	...
24	150	Bare	250	1.18	.96	18.2	3.6	.66	....	...	...	....	...	...
14	50	Bare	80	...	...	....	...	...	....	...	...	....	...	...
24	300	Holophane	250	2.16	2.08	17.0	3.9	.98	12.5	2.3	1.34	....	...	...
14	50	Holophane	80	...	...	...	...	...	7.0	.8	1.01	....	...	...
Average	..	.....	..	...	...	17.0	3.9	.98	8.1	2.0	1.32	3.6	1.8	1.48

of almost any series of tasks performed under properly controlled conditions is regarded by many as an index of attention, the higher mean variation indicating the lower degree of attention.[1] Thus a larger figure in the table might indicate a higher degree of attention under the non-uniform system.

The table shows that in every case the auditory choice reaction time is shorter under the non-uniform equipment and on the average is 17 per cent. shorter. The differences are all greater than twice the probable error of difference and average nearly four times as great. The differences between the per cent. mean variation under uniform and non-uniform lighting is small. Attention, as indicated by a tachistoscopic test, is superior under non-uniform illumination in 26 out of the 31 series. The average superiority, including the negative values, is 8 per cent. The differences are on the average twice the probable error of difference. The mean variation is considerably greater, about 30 per cent. on the average, under the uniform condition. In motor coördination three fourths of the series show greater efficiency under non-uniform lighting, with an average superiority, including negative values, of 3.6 per cent. The differences are on the average nearly twice the probable error of difference. In most cases the mean variation is greater under the uniform conditions—48 per cent. on the average.

The above results are more briefly indicated in Table II. The successive columns give the number of series performed under each of the three methods, the per cent. of that number in which the non-uniform series is superior, the average superiority of the non-uniform, the per cent. of series in which the greater mean variation (*i. e.*, lower degree of attention) is shown under the uniform lighting and the average of the quotients obtained by dividing the mean variation of the uniform by that of the non-uniform. The general tendency in favor of a quicker reaction and coördination and a higher degree of attention under the non-uniform equipments is evident.[2]

[1] Compare Pillsbury, "Attention," p. 89ff.

[2] Simple auditory reactions were tried with two subjects and yielded results similar to those with choice reaction. These two types were correlated later in the laboratory work.

TABLE II

	No. of Series	% of Series in Which Non-uniform Superior	Average Superiority Non-uniform, %	% of Series in Which Uniform Had Greater M.V.	Av. M.V. Uniform / M.V. Non-uniform
Choice reaction	18	100	17.0	50	.98
Tachistoscope	31	84	8.1	81	1.32
Motor coördination	22	77	3.6	82	1.48

IV. *Checks*

A number of other variables that might have been instrumental in producing the above results were ruled out by suitable check experiments.

As a check on the possible influence of the nature of the street in distracting elements tests were made on a few subjects with identical equipments on the two streets. The average differences were 6 per cent. and 2 per cent. on reactions and on the tachistoscope respectively, insignificant in comparison with the averages of Table I. Moreover the uniform equipment was sometimes on the north street and sometimes on the south street with little difference in results. Further, the same street was occasionally employed for all four series on a given evening. Here the sidewalk, traffic and window lighting conditions were constant, the only variable being the illuminating conditions. The average superiority of the non-uniform equipments under such conditions differs only slightly from the averages of Table I.

As to the type of illuminant used and the possible effect of Edision *vs.* Holophane units, some reaction series were made, using bare lamps in one or both streets. A street equipped with Edison units, compared with one identical but minus the globes yields slight differences in reaction. But if the Edison equipment is compared with bare lamps with refractors giving uniform illumination, or if two systems of bare lamps are used, one with refractors, the usual differences occur.

As to the intensity of illumination, the photometric data shows that in approximately half of the comparisons the mean intensity of the non-uniform system was greater than that

of the uniform and in the other half the reverse was true. The experimental results favored the non-uniform lighting under both conditions.

The time error was checked as above indicated by working on both equipments on a given evening in both time orders. Further, the uniform equipment was tested on the first series in about half of the comparisons.

As to the possible influence of number of lamps, cross street location, height or spacing, 250 c.p. lamps in Edison units were centrally hung 24 ft. high at 150 ft. spacing and compared with similar lamps in Holophane units with refractors having exactly the same mounting. The results are included in Table I. in the third horizontal group—6 series in choice reaction and motor coördination and 8 on the tachistoscope. In a few of these series moreover the two equipments were on the same street. In this rather crucial check it is evident that the usual tendency is manifest to about the usual degree.

Finally the question might be raised as to whether the superior results under non-uniform illumination are not due to a sort of voluntary alertness produced by the difficulty in seeing objects in the dark areas, that is, whether the results are not due to the dimness of the lighting rather than the non-uniformity. As a partial answer one subject was tested in reaction times on a uniformly lighted street run at the usual 7.5 amperes and then at 6.1 amperes. The reactions were 16 per cent. quicker under the former condition. This would indicate that the dimness factor was not responsible for the results of Table I.

V. *Degrees of Non-uniformity*

The above results point rather strongly to the superiority of non-uniform illumination of the types studied in its effect upon attention and reactions of the pedestrian. Whether the degree of attention varies with the degree of non-uniformity is doubtful. In the present study there were two non-uniform equipments that were used for the most part, with

150 ft. and 300 ft. spacing, the latter of which was more non-uniform in both horizontal and vertical illumination. Both of these equipments were frequently compared with a uniform system which might be regarded as the standard. The 300 ft. spacing shows a greater average superiority over the uniform system in choice reaction than does the 150 ft. spacing, but in motor coördination and attention this condition is reversed. A few tests were made comparing directly the two non-uniform equipments on the same evening with the same subjects. The 300 ft. system was slightly superior by all three methods.

This problem of degrees of uniformity could be answered only by exhaustive investigation. Even if the degree of attention were found to be a positive function of the degree of non-uniformity, it is probable that beyond a certain point the dimly lighted stretches of street would be so broad that the effect would amount to uniformity and produce retardation.[1]

C. Experiments in the Laboratory.

In transferring the problem to the laboratory there were five questions in mind:

1. How far do results obtained with the factor of uniformity and non-uniformity of illumination controlled in the laboratory substantiate in general the results obtained on the street?

[1] The results of the experiments carried on at the same time by other investigators to determine the revealing qualities of the different equipments for surface irregularities and obstacles in the street, tend somewhat in the same direction as the above results, rather than in the opposite direction. Targets of wood 2 inches thick or of galvanized iron plate the size of the surface of a paving block, painted gray and sanded were planted systematically through the two streets. Subjects walked along the traffic line at their normal rate, recording on a pad all the targets they saw and their location. It was found in general that these targets were perceived somewhat more readily on the street with the large illuminants widely spaced than on that uniformly lighted by small illuminants closer together. Furthermore with the equipments studied the targets were found almost as successfully in the darker areas between lamps as in the lighter areas. In other tests sections of stove-pipe one foot high, painted gray and sanded, were planted at various positions and the subjects in an automobile rode through the streets at constant rate with a device which recorded the maximum distance at which each target was seen. Results thus obtained tend in general in the same direction as those with the surface targets. The above results are not as yet published and no conclusions should be drawn from them, but their general tendency is of interest in the present connection.

2. How far do other kinds of reaction correlate with auditory reactions under the various illuminating conditions?

3. What are the effects of different degrees of non-uniformity?

4. Can the results in favor of non-uniform illumination on the street be attributed to greater voluntary effort in watching for obstacles in the dark regions between lamps, *i. e.*, does voluntary attention to one stimulus quicken reaction to another?

5. Can the results be attributed predominantly to the changing illumination through which the subject walks or to the light and dark patches on the street surface?

I. *Equipment*

The experiments were performed in an interior room with black walls, indirectly illuminated by light reflected from the white ceiling. The source was about 1.5 meters from the center of one end of the room directly above the head of the subject, and consisted of a 100-watt tungsten lamp in a box 60 cm. square and 45 cm. deep suspended 60 cm. from the ceiling. The subject sat alone, the experimenter and all the apparatus being in an adjoining room in front of the subject.

With the pedestrian walking on a non-uniformly lighted street there are two possible factors which may influence his mental state—the varying illumination intensity through which he is passing and the alternating light and dark regions on the street surface before him. To reproduce the first of these a 110-point circular rheostat was placed in series with the lamp illuminating the room. An alternating-current motor, geared down by belts and speed reducers, was arranged to drive a crank. One end of the crank was connected by a shaft with the margin of a wheel of greater radius, so that the latter oscillated through 160 degrees. This was belt-geared to a disc attached to the axle of the rheostat. Thus, when the motor was in operation the illumination in the room where the subject sat was slowly dimmed to a definite point (not, however, to the point where the color of the light changed per-

ceptibly), then increased and this process continued. These conditions roughly parallelled those to which the individual on the street is subjected as he walks along, the brightest point in the cycle in the laboratory corresponding to the position under a street lamp, the darkest part of the cycle to the position midway between lamps and the intervening points in the cycle to the intermediate positions on the street. The speed reducers were combined to produce complete cycles of 25, 55 or 85 seconds.

To reproduce the second factor, the shadows on the street surface, a black box with light interior 115×35×20 cm. with one of the 115×20 edges open was mounted vertically on a table at the subject's right with the open edge facing the left at an angle of 60 degrees to the wall. In strips attached to the edges of this side a piece of cardboard 115×20 cm. with 17×5 cm. holes in it 5 cm. apart could be moved up and down. In the center of the opposite side of the box was a 25–watt tungsten lamp. Thus a slightly fan-shaped array of shadows was cast on the wall directly in front of the subject. The cardboard was weighted at the bottom and attached at the top to a cord which ran through pulleys over the partition into the adjoining room. The cord could be attached to the crank which actuated the rheostat and the shadows thus be made to move up and down at various rates through a distance of 40 cm. This roughly reproduced the slowly changing effect of the light and dark patches on the street surface as the subject walks along under non-uniform lighting.

To parallel the uniform conditions on the street, the room was left at its normal condition with the lamp at maximum intensity. The average brightness on the black wall in front of the subject was then .119 meter candles.[1] When the light was dimmed to the lowest point of the cycle the average brightness was .037 m.c. With the shadows the average figure (averaging maxima and minima of the shadows and of the light spaces between them) was 1.38 m.c. When the shadow equipment was compared with a uniform, the card

[1] Photometric measurements were made over an area of approximately one square meter with a Lummer-Brodhun photometer, using an amyl-acetate lamp as a standard.

in the latter case was drawn up above the box and the light in the box shunted through the rheostat and held constant at a point which gave approximately the same average brightness on the wall. A point was found on the rheostat which gave a brightness of 1.42 m.c. as compared with the 1.38 m.c. for the shadow equipment run at the full current.

II. *Method.*

Experiments were made under these conditions upon various sorts of reaction times and upon attention as involved in a tachistoscopic test similar to that employed in the street experiments. For the latter purpose a portion of the wall between the subject and the experimenter was replaced by a piece of black cardboard 66 × 100 cm. in the center of which was a piece of ground glass 10 cm. square. Behind this was a rack into which paper or cardboard material could be inserted. Arrangements of geometrical figures in combinations of five, quite similar to those described above (p. 158) were employed. The figures were 4 sq. cm. in area, drawn with a soft pencil on white linen paper. When placed in the rack pressing against the ground glass and illuminated by a lamp 20 cm. distant they were plainly visible to the subject on the other side of the glass. The exposures of one second were made by covering the end of one of the speed reducers with a wooden disc with a sector of proper magnitude removed and arranging a contact of spring brass to press on the disc. The spring contact and the speed reducer were connected to two poles of the lamp circuit which was thus closed during a part of the revolution of the reducer. A telegraph key in the circuit enabled the experimenter to make the exposures whenever he wished. After the exposure the subject reproduced the figures on a small square of paper on the table at his side and placed his drawing in a drawer which the experimenter pushed through the wall for the purpose and which was then withdrawn. In scoring, a figure in the correct position counted 2 and a figure displaced less than a quadrant or disoriented counted 1. A different permutation of objects was shown each trial during the first series on two lighting equipments

in one time order, and then the same material repeated on the two equipments in the opposite time order. That is, each pattern was used once on each of the two equipments under comparison, thus eliminating any error due to degrees of difficulty in the material.

Auditory choice reactions with the feet were sometimes intermixed with the short-exposure tests and sometimes used alone. Keys were made of two pieces of wood 20×10×1 cm. hinged at one of the longer edges, with brass strips for contacts at the outer edges and held apart by light springs. Each foot rested on one of these keys, the normal weight of the foot being sufficient to close the circuit. The stimuli consisted of a small electric gong and a sounder made by removing the bell from a larger gong so that the armature struck the magnets. The time was measured with two electric vernier chronoscopes. By the use of a double-throw switch, the two chronoscopes could be operated through the same connections and thus could be used in quick succession and counted simultaneously. The counting was facilitated by giving the second stimulus on the 10th, 15th, etc., swing of the first. The chronoscopes were always callibrated at the beginning of the hour. For simple reactions a telegraph key on the table beside the subject was connected in parallel with the foot contacts.

For the stimulus for a simple visual reaction a battery lamp on a frame, so hinged that it could be swung near the ground glass before the eyes of the subject, was placed in parallel with one of the bells by means of a double-throw switch, and a piece of cardboard with a 6 mm. round hole placed between it and the glass. The subject thus received a small patch of light as a stimulus.

In conducting the experiment, two equipments were always employed on a given hour in both time orders, *i. e.*, uniform, non-uniform, non-uniform, uniform, or the reverse. The subject sat passively under the given equipment for approximately 3 minutes prior to each series. On a given comparison half the subjects began with the uniform and half with the non-uniform equipment, and when the same subject worked

two different hours on a given comparison, the second hour began with the equipment the opposite of that of the first hour.

The instructions for the short-exposure test (after a preliminary explanation of the material and observation of a sample exposure) were, "Reproduce as many of the figures as possible in the correct position." Those for the choice reaction were "When you hear the bell lift the right foot and when you hear the sounder lift the left." When the reactions were intermixed with the short-exposures the instructions were a combination of these two: "When you hear the bell lift the right foot, when you hear the sounder lift the left, and when you see the pattern reproduce as many of the figures as possible in the correct position." For the simple reactions the subject was instructed: "Motor reaction to the bell" or, "Motor reaction to the light." The instructions were repeated before each of the four series during the hour. At the beginning of the hour three patterns and ten reactions of the sort to be used were given for warming up, and the first two reactions of the four series were omitted from the results.

The number of trials varied according to the combination of tests used on a given hour. The tachistoscope tests were always given in series of 40, *i. e.*, 20 on each equipment. When choice reactions were used in connection with the tachistoscope there were 80 trials, 40 with the short exposures intermixed and 40 without. With simple reactions it was possible to give 240 per hour, usually 120 auditory and 120 visual.

The reactions and exposures were given at irregular intervals. The order for the choice reactions was determined by shuffling cards containing the same number of right and left at the beginning of each series. When the reactions were intermixed with the tachistoscope, cards marked "T" were intermixed with the others before shuffling.

The subjects were graduate students in the Harvard Laboratory and a few undergraduates doing advanced work in psychology. None of them knew the trend of the results of the experiments on the street. Ten subjects participated at different times.

III. *General Results*

Various combinations of uniform and non-uniform illuminating conditions were studied comparatively and the results tend in general to substantiate those obtained on the street. This can best be shown by averaging together all those series in which a distinctly non-uniform condition was compared with a uniform. Table III summarizes the larger part of such results. The Roman numerals at the left indicate the equipments compared and are as follows:

I. Non-uniform.....26 sec. dimmer.
Uniform.....Dark wall.
II. Non-uniform.....26 sec. dimmer—shadows stationary.
Uniform.....Dark wall.
III. Non-uniform.....26 sec. dimmer—26 sec. shadows.
Uniform.....Dark wall.
IV. Non-uniform.....26 sec. dimmer—26 sec. shadows.
Uniform.....Light wall.
V. Non-uniform.....55 sec. dimmer.
Uniform.....Dark wall.
VI. Non-uniform.....55 sec. dimmer—shadows stationary.
Uniform.....Light wall.
VII. Non-uniform.....55 sec. dimmer—55 sec. shadows move.
Uniform.....Dark wall.
VIII. Non-uniform.....85 sec. shadows.
Uniform.....Light wall.
IX. Non-uniform.....85 sec. dimmer—85 sec. shadows.
Uniform.....Light wall.
X. Non-uniform.....Shadows stationary.
Uniform.....Light wall.
XI. Non-uniform.....25 mm. shadows stationary.
Uniform.....Light wall.

The first column under each heading gives the average per cent. of superiority of the non-uniform equipment (*i. e.*, shorter reaction time or higher score on the tachistoscope, per cents. reckoned in terms of the smaller of the two averages), for the given comparison. Below this figure is in each instance the total number of subjects who yield differences in favor of the non-uniform equipment followed by the total number of subjects who participated on that comparison. The second column gives the average obtained by dividing the differences by the probable error for the various subjects,

TABLE III

	Tachistoscope			Auditory Choice With			Auditory Choice Without			Simple Auditory			Simple Visual		
	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform
I.....	2.16	1.27	.99	.94	1.63	1.00	1.35	1.60	.96	.90	1.46	1.04	7.07	3.16	1.07
	8/11	...	...	6/11	...	...	8/11	...	...	3/5	...	...	4/5	...	...
II.....	−2.50	2.59	1.04	9.05	3.03	1.14	6.86	2.51	1.06	...	...	...	....	...	...
	2/6	...	...	5/6	...	...	5/6	...	...	...	...	...	....	...	...
III.....	−2.30	1.22	1.14	2.21	1.55	1.10	2.28	2.53	.95	...	...	...	....	...	...
	4/8	...	...	5/8	...	...	3/8	...	...	...	...	...	....	...	...
IV.....	....	...	...	...	...	...	...	...	...	5.75	2.15	1.00	2.70	3.20	1.30
	....	...	...	...	...	...	...	...	...	2/2	...	...	1/2	...	...
V.....	5.79	1.62	1.08	.76	1.55	.95	6.36	1.87	1.12	...	...	...	....	...	...
	7/11	...	...	8/11	...	...	6/8	...	...	...	...	...	....	...	...
VI.....	....	...	...	...	...	...	...	...	...	5.36	2.22	.95	6.46	3.88	1.20
	....	...	...	...	...	...	...	...	...	5/6	...	...	5/6	...	...
VII.....	....	...	...	...	...	...	2.12	1.45	1.11	5.01	1.27	1.15	....	...	...
	....	...	...	...	...	...	4/5	...	...	5/5	...	...	....	...	...
VIII....	....	...	...	...	...	...	2.86	1.64	1.02	8.65	3.26	1.04	....	...	...
	....	...	...	...	...	...	4/6	...	...	4/6	...	...	....	...	...
IX.....	....	...	...	...	...	...	3.35	1.56	.92	5.55	2.49	1.11	−6.10	2.68	.98
	....	...	...	...	...		3/4	...	...	7/8	...	...	1/4	...	...
X.....	4.64	1.58	.95	2.37	.91	1.68	.83	1.42	1.10	7.99	2.80	1.15	....	...	...
	4/9	...	...	2/4	...	...	7/14	...	...	9/10	...	...	....	...	...
XI.....	.52	1.02	.93	...	...	...	...	...	...	3.35	1.53	.95	....	...	...
	2/4	...	...	...	...	...	...	...	...	...	3/4	...	....	...	...
Average .	1.91	1.52	1.01	2.50	1.75	1.09	2.88	1.72	1.04	5.76	1.73	1.07	3.24	3.30	1.12
	27/49	...	...	26/40	...	...	40/62	...	...	38/46	...	...	11/17	...	...
	55%	...	...	65%	...	...	65%	...	...	82%	...	...	65%	...	...

and the third gives the average of the figures obtained by dividing the per cent. mean variation of the uniform series by that of the non-uniform for each subject. To illustrate: the figures at the beginning of the table indicate that for the tachistoscopic tests on the comparison denoted as I., of the 11 subjects, 8 yield differences in favor of the non-uniform equipment with an average superiority of the non-uniform (including negative values) of 2.16 per cent. The average of the 11 differences divided by their probable error is 1.27. If the per cent. mean variation of the results on the uniform equipment is divided by that for the non-uniform the average of these figures is .99.

The summaries at the bottom of the columns give in the first row the weighted averages of all the first rows above, obtained by multiplying each average by the number of series involved and dividing the sum of these by the total number of series. In the second row of the summary are the sums of the corresponding figures above, *i. e.*, the number of series yielding differences in favor of the non-uniform, the total number of series and the per cent. which the former is of the latter.

From the averages it is evident that there is a general tendency toward greater efficiency under non-uniform illumination. Simple auditory reaction time is superior under such lighting in 82 per cent. of the series an average of 5.8 per cent. Simple visual reaction time is quicker in 65 per cent. of the series, an average of 3.2 per cent. Auditory choice reactions with tachistoscopic trials intermixed yield similar results in 65 per cent. of the series with an average superiority of 2.5 per cent. Auditory choice reactions without the tachistoscopic trials tend in the same direction in 65 per cent. of the series, an average of 2.8 per cent. And the tachistoscopic experiments tend likewise in favor of non-uniform illumination in 55 per cent. of the cases with an average superiority of the non-uniform of 1.9 per cent. The differences in general are between 1.5 and 2 times the probable error of difference and in the case of the visual reactions over 3 times. The mean variations do not show as marked a

tendency, although they average in each method slightly in favor of the non-uniform condition. It might be noted that the individual averages in the simple reactions are based on a larger number of trials and the differences prove greater.

At the end of each hour the subjects were asked whether they had any introspection as to their comparative mental state under the two systems of illumination. In a large majority of the series (83 per cent.) the subject had nothing of interest to report. The usual statement was: "Felt no difference under the two systems"; or "Think I did as well under one system as the other." It is evident that an individual may be influenced by the illuminating conditions without being aware of it. However in the cases in which introspection was given there is no unanimous tendency. Only two of the subjects are at all consistent in their reports, the others feeling more efficient sometimes under uniform and sometimes under non-uniform lighting. There is no general tendency in the introspective as in the objective results. However the two correlate in about two thirds of the individual cases in which introspection is given—a report that the subject is less efficient under a given system corresponding to a slower reaction time. There are no marked individual differences, *i. e.*, subjects who are consistently superior under uniform or non-uniform conditions. On the whole, considering the few cases in which introspection was forthcoming and its variable character, the principal stress must be placed on the objective results. It is evident that a subjective estimate of the mental state under various conditions of illumination is sometimes unreliable, and furthermore such illuminating conditions frequently influence the reactions without producing any conscious effect at all.

Thus the results obtained in the laboratory tend to corroborate those found on the street although not as marked. Reactions are quicker and a higher degree of attention is maintained under non-uniform illumination. It would seem that the results could be attributed solely to the non-uniformity factor either in the form of the increasing and decreasing intensity or of the shadows of the visual field. External

factors such as traffic and extraneous light from store windows were not present in the laboratory. Nor could anything be attributed to the type of lamp or the number and mounting of lamps. Further, when a shadow equipment was used, the brightness of the wall in the corresponding uniform equipment was in some series equated to the average brightness of the former. The results are evidently due to the uniformity factor.

IV. *Correlation of Other Reactions with Auditory Choice*

In the experiments on the street auditory choice reactions were used almost entirely. From Table III, it is evident that in the laboratory simple auditory reactions as well as auditory choice yield greater efficiency under the non-uniform illumination. Data is available for 20 series in which the two were measured on the same hour with the same subjects. If the individual per cents. of superiority of the non-uniform equipment are ranked for the simple and the choice reactions, the two correlate by the method of rank differences to give a coefficient of $.42 \pm .13$. It is to be noted further that simple visual reactions also tend on the average in the same direction as the others. Scarcely enough series are available in which auditory and visual were measured on the same hour to compute correlation on the basis of individual averages, but the general tendency is manifest.

V. *Degrees of Uniformity*

Given the general superiority of non-uniform illumination, the question arises as to various degrees of non-uniformity. In the laboratory as on the street no conclusive answer can be given. In Table III. are a few cases in which results are available under two comparisons that differ merely in their temporal aspect. Combinations I. and V. compare the 26 and 55 sec. dimmer respectively with the constant conditions. The 55 sec. appears considerably superior in tachistoscope experiments and in auditory choice reactions alone, and slightly inferior in auditory reactions intermixed with the tachistoscope. Combinations III. and VII. compare 26 and 55 sec. cycles of the dimmer and shadows moving together,

with the constant conditions. The average difference in auditory choice reaction is slight, but a larger per cent. of the series yield superiority of the non-uniform system in the case of the 55 sec. cycle. The 85 sec. cycle (IX.) of dimmer and shadows, compared however with the light wall equipment, is superior to either the 26 or the 55 sec. cycles. One might also note the comparative results under combinations II. and III. in both forms of choice reaction. With the 25 sec. dimmer and stationary shadows the score is high, but when the shadows move in 26 sec. cycle as well as the dimmer, the score is low. Evidently the rapidly moving shadows offer a distraction.

Some series were performed to test this problem directly. The subjects were given simple auditory and visual reactions under the dimmer moving at three rates on a given hour with the time-error properly controlled and at another time under the shadows moving at three rates. The results indicate that the slower rates are more advantageous with the shadows and the more rapid rates with the dimmer for auditory reactions, while visual reactions are facilitated by the more rapid rates with both.

The slower rates with the dimmer and shadows correspond roughly to the more non-uniform conditions on the street, *i. e.*, to a system in which the subject walking along is less frequently under a lamp and in which the light and dark patches on the street are broader and hence produce a less rapid change in the visual field. There are slight indications in the results on the street of greater efficiency in auditory reactions the more non-uniform the lighting. In the laboratory from the standpoint of the changes in the visual field, auditory reaction is likewise quicker the more non-uniform the system, but from the standpoint of change in illumination intensity this is not the case.

On the whole no definite conclusions can be drawn upon the present problem. It seems best to conclude on the basis of both street and laboratory experiments that within reasonable limits no marked differences in efficiency are found under various degrees of non-uniformity of illumination,

but that the important point is the general superiority of the various non-uniform equipments to the uniform.

VI. *Voluntary Alertness*

The question was raised in the street experiments as to whether the superiority of reaction under non-uniform illumination was not due to a voluntary alertness produced by the necessity of watching more carefully for obstacles in the darker regions between lamps. It was endeavored in the laboratory to answer this question as to whether voluntary attention to one stimulus under the various illuminating conditions would quicken reaction to another. For this purpose auditory choice reactions were given when the subject's attention was somewhat focused on the patch where the tachistoscopic material was to appear, followed by auditory choice reactions with no exposures intermixed. Series were performed on five different illumination comparisons, numbers I., II., III., V. and X. (see p. 173). 80 trials were given on a comparison. The results for the uniform and the non-uniform equipments were then evaluated separately and the efficiency of auditory choice reactions presented alone compared with that of such reactions with tachistoscopic trials intermixed. The former were found to be superior with the majority of the subjects under all the lighting equipments thus investigated. Considering the totals under the five uniform equipments, 76 per cent. of the series yielded a superiority of the reactions presented without the tachistoscope, with an average superiority (including negative cases) of 5.2 per cent. and the average difference 2.44 the probable error of difference. Considering the totals under the five non-uniform equipments, 76 per cent. of the series likewise yielded a superiority of the reactions presented without the tachistoscope with an average superiority of 7.9 per cent. and an average difference 2.6 the probable error. Evidently voluntary alertness to one stimulus under the various illuminating conditions does not quicken reaction to another stimulus.

VII. *Shadows vs. Changing Illumination Intensity*

Finally there remains the question as to whether the effect of non-uniformity on the street is due primarily to the changing illumination through which the subject passes as he walks along the street or to the light and dark patches on the street surface. One series was performed to test this directly, auditory choice and simple reactions being given under the 55 sec. dimmer and under the stationary shadows. Choice reactions proved quicker under the dimmer with 3 of the 5 subjects, with an average superiority of 9 per cent. and simple reaction was quicker for 3 subjects under the shadows with the average difference small. Nothing in the introspection throws light on the matter.

In Table III. if we consider the simple auditory reactions we find the highest comparative efficiency for the non-uniform lighting on comparisons VIII. and X. in which the shadows are alone at the 85 sec. cycle and stationary respectively, but the results with a combination of the shadows with the dimmer are not greatly inferior to these. On choice reactions without the tachistoscope the highest scores are for II. and V. which involve the 26 sec. dimmer with stationary shadows and the 55 sec. dimmer respectively. In these cases, however, the uniform system used involves the dark wall with a possibility of the influence of the brightness variable. Of the others the highest score is for the 85 sec. dimmer with the shadows moving (IX.), which is superior to that for the shadows alone either stationary (X.) or in an 85 sec. cycle (VIII.). Choice reactions intermixed with the short exposures show the highest score for II. a combination of the 26 sec. dimmer and stationary shadows and the next highest for X., the stationary shadows. The tachistoscope shows a high score for both V. and X., the 55 sec. dimmer and the stationary shadows. On the whole, non-uniform illumination produced by the dimmer seems slightly superior to that produced by the shadows in its effect upon choice reaction, while the result is reversed with simple reaction, and a combination of dimmer and shadows seems in general to produce better results then either alone. Presumably the effect of

non-uniform street illumination upon attention and reactions is due to both factors, the changing illumination intensity through which the individual passes and the dark regions on the street surface.

D. Conclusions

The results of experiments upon subjects walking under actual conditions of street illumination indicate rather consistently that the auditory choice reaction time is quicker, the attention more alert and the efficiency in motor coördination greater under a distinctly non-uniform illumination than under a distinctly uniform. There is no other variable that follows as directly with the results as does the uniformity variable. Sometimes the non-uniform street has more distracting elements in the form of people on the sidewalk or extraneous light from stores, sometimes less; sometimes its average illumination intensity is greater, sometimes less; sometimes it has a different type of illuminating unit, sometimes the same; sometimes it constitutes the first of the series, sometimes the second; sometimes it has fewer lamps with different mounting and spacing, sometimes the same number of lamps with the same mounting and spacing. But under all these conditions it shows a superiority over the uniformly lighted street. When the uniformity factor is a variable in the comparative study of two equipments, there is a marked difference in results, and when it is a constant the difference is slight. Under a distinctly non-uniform street illumination as compared with a uniform, the subjects show in 77 per cent. of the series a superiority in motor coördination with an average (including negative values) of 3.6 per cent. superiority; in the tachistoscopic test they are superior in 84 per cent. of the series with an average (including negative values) of 8 per cent.; and in auditory choice reaction every series yields a quicker reaction time with an average of 17 per cent. superiority.

With the problem transferred to the laboratory and non-uniformity produced by slow rhythmic increasing and decreasing of the illumination intensity of the room, or by shadows to break the monotony of the dark wall—conditions

designed to parallel respectively the changing illumination intensity through which the individual walks on a non-uniformly lighted street, and the shadows on the street surface—the results tend in general to corroborate those just mentioned. Further, simple visual and auditory reactions correlate with choice reactions in this tendency. The superiority of reactions under non-uniform lighting on the street cannot be attributed to a voluntary alertness produced by the necessity of watching more carefully for obstacles in the darker regions between lamps, for the experiments show that voluntary attention to an expected stimulus under such illuminating conditions does not quicken reaction to another stimulus. Within reasonable limits no definite relation exists between degree of efficiency in reaction and degree of non-uniformity in illumination, the important point being the general superiority of the various degrees of non-uniformity to the uniformity. The superior effect of the non-uniform street lighting seems due to both the changing illumination intensity through which the individual walks and the light and dark regions on the street surface.

Finally the practical implications of the experiment should be borne in mind. Safety on the street depends largely on the ability to apprehend a dangerous situation and carry out quickly the proper psychomotor response. If external conditions produce a lower degree of attention and a retardation of reaction, the danger of such conditions is manifest. The above experiments show that uniform street illumination as compared with a moderately non-uniform, produces such a state in the case of the pedestrian walking through the street. The work in the laboratory with various degrees of non-uniformity would seem to indicate a similar condition in the case of more rapid movement through the street such as that of the driver or chauffeur. Driver and pedestrian alike must make quick decisions and reactions and a difference in reaction time of a few hundredths of a second may avert or precipitate an accident. The conclusion thus seems warranted that, ceteris paribus, non-uniform illumination is more conducive than uniform to safety on the street.

DISCUSSION

TONAL VOLUME AND PITCH

In J. G. Rich's paper: *A Preliminary Study of Tonal Volume* (this JOURNAL, February), he makes the assumption that "If they (pitch and volume), are two names of one and the same attribute, as Dunlap asserts, they will have the same limen." I have already pointed out (in my *System of Psychology*, pp. 143–4), that we may reasonably expect that the opposite principle will hold. It is very possible that the pitch-discrimination of the unmusical person, and sometimes of the musical person where the differences are large, is a pure extensity judgment, *i. e.*, based on the extent of the stimulation in the hair-cell series; but that the discrimination of the musical person, especially where the differences are small, is primarily a local-sign judgment, *i. e.*, based on the differences in the hair-cells at which the region of stimulation ends. Mr. Rich's results may therefore be more reasonably interpreted as supporting the extensity theory than as against it, although I can not as yet see that these results have any certain bearing on the theory. There are so many possibilities as to the contential nature of the judgment-reactions in these experiments that it seems hardly worth while speculating about them.

I had hoped that we would be able to exclude the extensity theory, as the frequency theory ('telephone theory') is so much simpler; but on the contrary the extensity theory becomes more and more plausible. The most vital objection to it has just been removed by the epoch-making work of Hardesty (*American Journal of Anatomy*, 1915, Vol. 18, 471–514), who has shown that a structure such as the tectorial membrane may be affected by vibrations of different periods in just the way indicated by the psychological facts; the slower rates affecting larger, the faster, smaller extents of the tectorial membrane, these extents coterminous at the 'vestibular' end.

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